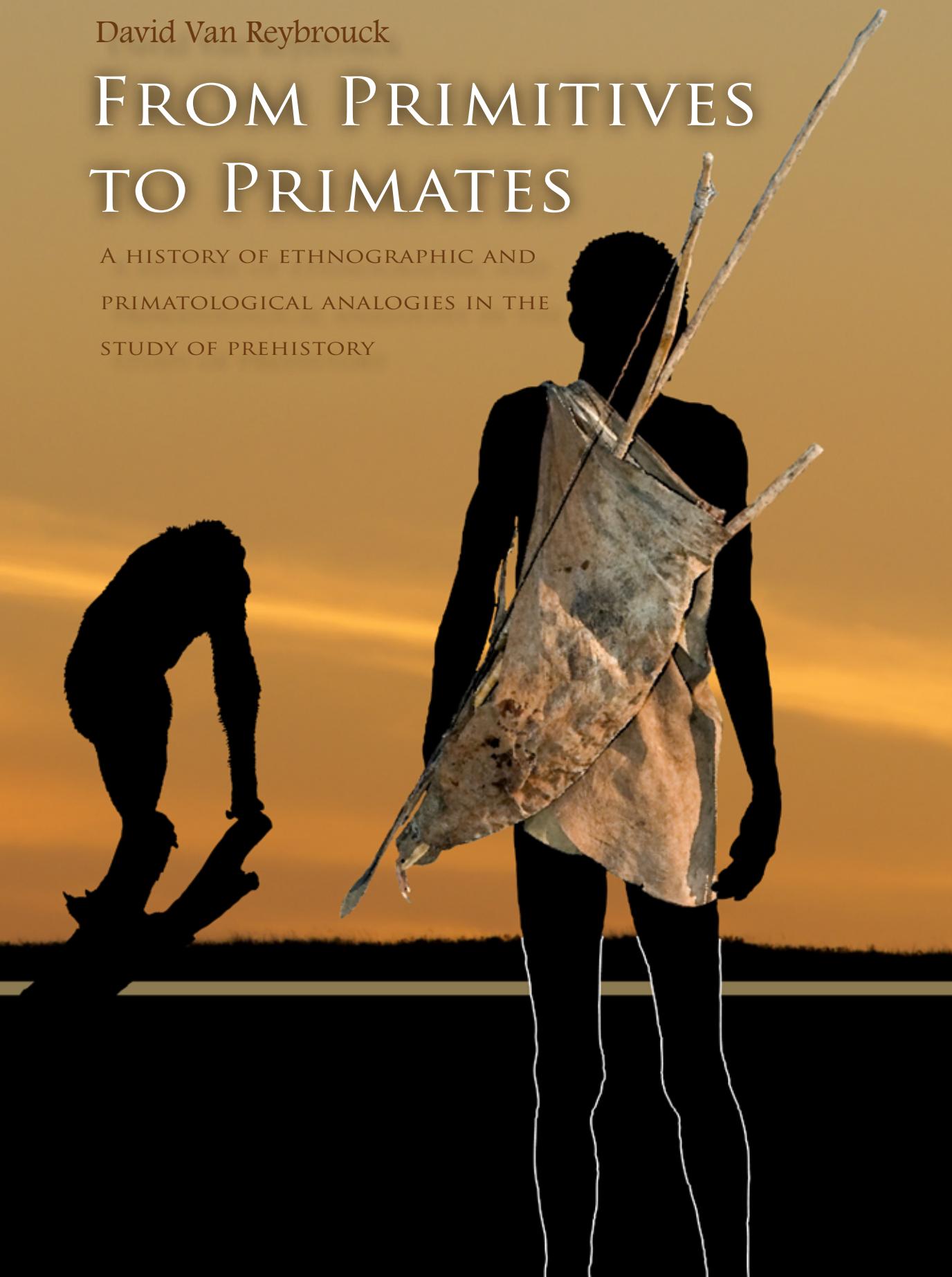


David Van Reybrouck

FROM PRIMITIVES TO PRIMATES

A HISTORY OF ETHNOGRAPHIC AND
PRIMATOLOGICAL ANALOGIES IN THE
STUDY OF PREHISTORY



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STUDY OF PREHISTORY

*Voor Bekie, Hadewich, Stefaan, Rose-Marie en Sebbe,
die stierven in de sneeuw.*

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Preface

'Though analogy is often misleading, it is the least misleading thing we have,' said the eighteenth-century moralist Samuel Johnson. This may be slightly frustrating for those who want to use analogy, for historians of science it makes analogical reasoning an interesting topic for research. Indeed, if analogy really oscillates between being helpful and being tricky; a history of analogy use in a specific discipline should become an exciting exercise.

Exciting this work has certainly been. During the years I have been working on this topic, my fascination for the ways by which new insights are crafted through parallels with other domains has rarely decreased. In a sense, analogical reasoning shows science from its most creative and artistic side. This is especially true for disciplines like prehistoric archaeology, primatology and human origin studies where it is often indispensable to make innovative claims about the remote past. I, therefore, hope that some of my personal enthusiasm about this topic and the pleasure I have experienced while studying it may transpire through the pages of the present study.

A text which attributes so much importance to the role of external sources in the construction of scientific knowledge cannot claim much autonomy of its own. Nor do I wish to do so. Instead, during the past years I have had the pleasure to work with a number of people who have helped me in one way or another. Wil Roebroeks' often impulsive criticism was counterbalanced by his impulsive enthusiasm; I have learnt from both. Raymond Corbey drew my attention to debates in the field of primatology. I got interested in them, much more than I would have thought possible. The participants in the Leiden Pioneer Project 'Changing Views on Ice Age Foragers' constantly reminded me of the difficulty of cross-disciplinary research, an invaluable insight when discussing the history of human origin studies. Olga Yates, the project's secretary, was a constant source of well-disposed helpfulness whose acute sense of wit made the profession quite enjoyable indeed.

Many scholars in the Netherlands and abroad have granted me the opportunity to discuss and exchange ideas, either 'live' or via e-mail. This has really improved my understanding of fields like history of science and primatology and I have expressed my sincere gratitude for their generosity to each of them. During my research, I was quite fortunate to get in contact with a group of primatologists related to the University of Antwerp and the Antwerp and Planckendael zoos. Linda Van Elsacker, Jef Dupain, Hilde Vervaecke, Ellen Van Krunkelsven and several others proved inspiring discussion partners whose interest has not ceased to surprise me. To Hilde, in particular, I am much indebted for organizing a wintry weekend in her country house in the Belgian Ardennes where, entrapped by snow and surrounded by bisons, a dozen of primatologists took the effort to discuss my chapter 4. It was one of the most stimulating moments of this research project.

Receiving comments on draft versions of the chapters has been an invaluable help to improve my text. Chapter 1 was read and commented upon by Jan Kolen and Peter van Dommelen. Ton Lemaire and Wiktor Stoczkowski gave useful feedback on chapter 2, whereas John Bintliff, Peter van Dommelen and Alexander

Gramsch advised me on how to improve chapter 3 or parts of it. Chapter 4 received not only valuable criticism from the ‘Planckendael gang’; Karen Strier also provided very constructive comments on the section related to the baboon model and with Jeanne Sept I had the opportunity to discuss the part on ethoarchaeology and the crisis of traditional modelling. Nathan Schlanger was brave enough to work his way through the entire manuscript and to spend an afternoon in the Jardin du Luxembourg discussing many aspects—I am much obliged to him for that. My English, finally, was checked by Karen Waugh.

On several occasions I was given the opportunity to exchange my ideas with students during lectures or seminars. In particular I would like to thank the participants and organisers of the master’s course on primatology in Antwerp and the doctoral course ‘Archeologie en Antropologie’ in Leiden. Lecturing gives one the pleasant illusion that one’s ideas finally belong to Knowledge; fortunately, students are there to question that.

I am also indebted to my former colleagues in AREA, the European research network on the archives of archaeology, for allowing me to occasionally escape the duties of scientific coordination in order to finish my PhD. Alain Schnapp and Sander van der Leeuw were not just understanding but even encouraging in that respect. Marianne De Baere at *De Morgen* also learnt to live with a free-lance writer who was ‘nearly ready’ with his thesis ‘but not quite yet.’

The first letter I received in my pigeon-hole after moving from Cambridge to Leiden was an invitation to join the editorial board of Archaeological Dialogues. This was six years ago and I am still a member of it. It is hard to express my sense of elation about having been able to work and to continue to work with such an inspiring and truly generous group of people. My doctoral research and indeed my entire stay in the Netherlands would have been very different without the ongoing discussions and personal support of my fellow-editors: Peter van Dommelen, Jan Kolen, Jos Bazelmans, Jan Slofstra, and since recently, David Fontijn and Fokke Gerritsen. Apart from intellectual exchanges, they have also assured me of a number of agreeable excursions and memorable nights.

Some people do not clearly fit any of the above categories and have nonetheless contributed, one way or another, to my work. I am indebted for enriching conversations to Dirk Jacobs, Marc De Bie, Paul Treherne, Christophe Abegg, Alexander Verpoorte, David Fontijn, Kaat Wils, and Froukje Slofstra. My parents and my brother have followed this doctoral research from a discrete distance, often wondering what it was all about and why it had to take so long. I felt nonetheless supported by their silent trust. My friends in Belgium and beyond allowed me to escape the worlds of reading and writing for a day of cycling, an evening for cards or a night of talking—they don’t realize how much I appreciated this. Dieke Wesselingh was there all along, and still is, even now that the shackles are shed.

Finally, most of this text was written after my first skiing holiday where I lost five very dear friends in the Cavalese gondola accident. I would like to thank my friends, dead or alive, for permanently reminding me of the value of friendship. The palm grows under pressure.

Brussels-Leiden, October 2000

Introduction

It was a pleasant morning when the Albertville moored in the port of Antwerp on the 27th of June 1897. The night before there had been excessive rainfall and a thunder-storm at four o'clock in the morning was described as 'unforgettable'. Large parts of the province of Antwerp were inundated and in the morning the temperature rose to 15 degrees Celsius. Aboard the ocean steamer, nearly three hundred Congolese were wrapped in woollen cloths, their heads enveloped with shawls. They had come all the way from the Free State of Congo, the private property of the Belgian king Leopold II. The monarch wanted them to be displayed at his colonial exhibition in Tervuren, near Brussels. There were well over a hundred soldiers and musicians, 93 Bangala, 27 Mayombe, 12 Basoko and two pygmies. For two months they occupied the bamboo huts near the pond in the park, paddled in their canoes and broiled their food. As icons of Africa's savagery and excuses for the king's relentless exploitation and missionary conversion, they played at being themselves before the curious and bewildered eyes of Belgian citizens.

The Tervuren colonial exhibition welcomed more than one million visitors, an astronomical figure even by modern standards. The Congolese villages served as the fair's principal attraction. Whereas July had eventually become warm and pleasant, August was wet and cold. Seven of the exhibited natives died of influenza; their sober graves are still to be seen at the Tervuren churchyard today. Due to the unusual rainfall, the path which led the visitors along the villages—they were not allowed to enter the settlements—had become so muddy that a ramp had to be constructed. The country's Francophone noblemen, their elegant but appalled wives, the nouveaux riches from the urban and industrial elite, the gangs of mocking children, the mumbling priests, the farmers and the workmen with their raw Flemish laughter, all these visitors could literally look down on the miserable creatures from the heart of Africa. All summer long, articles in the national press on the Congolese stressed their absence of civilization, their lack of religion, and their inclination towards cannibalism. Their smiles were called 'simian', their aspects 'hideous', their blood 'boiling'. 'This is the famous "civilization" of the negroes! And these here were even selected! Eh bien, merci...' Another reporter wondered how it was possible that people who had been brought by Christians to a Christian country still lived 'in the darkness of the rudest fetishism'. One journalist even warned against the fraternal exchange of blood with blacks which might invoke cannibalism: one individual who was said to have done so, had already bitten his mother-in-law when she climbed the stairs and he had devoured the flesh 'comme un orateur socialiste'.

Interestingly, the epithets used to stigmatize the Congolese natives—lack of reason, ugliness, brutality—were the very same applied to prehistoric savages which archaeologists had begun to unearth. The pygmy and the Neanderthal both evoked the same strange mixture of curiosity and repulsion. In an exhibition where the triumph of Western progress and the propaganda for white supremacy was so unabashed, the representations of the primitive Congolese echoed the discourse on our primeval ancestors. Were those exotic exemplars of the human race not comparable to the savages which

had once roamed the Belgian Ardennes? Had the cave site of Spy not furnished us, less than ten years ago, with two fossil human skeletons with equally protruding faces? Had the valleys of the rivers Meuse and Lesse not yielded numerous simple implements of prehistoric cave-dwellers? Perhaps the association was not literally made, but the surrounding discourses on primitivity and the representations of the others' wildness were convergent. Looking down from the ramp on the primitive tools the 'Basoko' and the 'Bangala' used, on their prognathous faces and their thick, curly hair, on the sagging breasts of the half-naked women, hearing the coarse and dark sounds of the cries they howled at each other, smelling the mud, the rotten reed, the fires and the scorched meat, more than one visitor of this exotic circus could have assented to Darwin's words when he first encountered the Fuegians: 'Such were our ancestors.'

After the exhibition was over, the site assumed again its rural Brabant aspect—but not for long. King Leopold decided to erect a permanent museum on the spot and Charles Girault, architect of the Petit Palais in Paris, designed a magnificent, if somewhat pompous, neo-classicist building with a garden inspired by Versailles. The complex was inaugurated in 1910, shortly after the king's death and shortly after Congo had become a Belgian colony. Once the stage of exotic scenes, Tervuren now became home to a ponderous bastion of colonial learning: the Congo museum. In the first half of the century, until Congo's independence in 1960, the museum made gigantic acquisitions, rendering it one of the world's richest collections on Africa to date. Thousands and thousands of beetles, stuffed mammals, rock samples and exotic seeds entered the museum gates, along with a titanic dug-out canoe, superb masks, wooden sculptures, and ritual garments. Natural history and cultural history, taxidermy and ethnography, wildlife and primitive life went hand in hand in this epitome of colonial ordering and mastery.

*The collections were so abundant that several new zoological species were described on the basis of them. These were not the least of animals but impressive creatures like the bright Congo peacock and the elusive okapi. On a late afternoon in 1928, the American primate anatomist Harold Coolidge, touring along European museum collections, picked up from a storage tray in Tervuren a skull which looked like a juvenile chimpanzee. He noted, however, that the epiphyses were fused, so that the animal must have been adult. Remembering the great differences he had seen between the two living pet chimps in the house of American psychologist Robert Yerkes, he started to wonder whether there was really just one species of chimpanzee. That very afternoon, just before closing time, Coolidge expressed his doubts to Henri Schouteden, the then museum director, who two weeks later spoke about it with Ernst Schwarz, a German anatomist. Schwarz was most intrigued, took some measurements on one skull and rapidly drafted together a paper, claiming that we were faced with a distinct subspecies of chimpanzees, the pygmy chimpanzee or *Pan satyrus paniscus*. In 1933, Coolidge took revenge for this scientific theft and published a fuller description of the animal, which he raised to the species level, *Pan paniscus*, the animal we now refer to as the bonobo. Fraudulence and rivalry laid thus at the discovery of the species which eventually appeared to be the most peaceful of all great apes.*

Although the discovery of the bonobo was one of the last and most spectacular attainments in mammalian zoology, the skeletons and type specimens in Tervuren remained virtually untouched for nearly half a century. In the summer of 1973, Adrienne Zihlman and Douglas Cramer, two American physical anthropologists with a strong interest in human evolution, came to Tervuren to study the bonobo bones again. Eighteen skeletons were retrieved from the shelves and they were measured in all possible ways. Zihlman, who strongly took part in the 1970s feminist movement, sought to replace the androcentric, baboon-inspired, Man the Hunter scenario of human evolution with a narrative that accorded a more active role to females. Long-time defending the common chimpanzee as the best alternative ancestral prototype, she started to replace it by the bonobo after she realized that sexual dimorphism was much smaller among bonobos than among chimps. Female and male bonobos were each other's equals, at least in anatomical respect. The skulls and skeletons kept in Tervuren were therefore not just interesting in their own right but echoed, she urged, australopithecine anatomy of several million years ago.

But not everyone agreed. Another American anatomist, Henry McHenry run his callipers and tape measure along the same morphological evidence when he was a visiting scholar in Tervuren some years later. Taking over 20,000 measurements, he strongly objected to the idea that the bonobo was more than any other primate privileged in clarifying our evolutionary past. On top of that, less than twenty-five kilometres north of Tervuren, in the Planckendael Zoo near Mechelen, behavioural studies on captive bonobos started to be undertaken from the late 1980s onwards. Whereas the spaciously housed Planckendael community grew into the world's largest social group of bonobos in captivity, primatologists who studied them refrained from drawing all too easy inferences on human evolution. Bonobos were no living fossils.

Back in Tervuren, Francis Van Noten, curator of the prehistory and archaeology section of the museum and professor at the Catholic University of Leuven worked on African and European prehistory. He received international acclaim through his excavations of the Epipalaeolithic settlement of Meer in Northern Belgium which set a new standard for analysing and interpreting seemingly poor sites from the sandy regions. Though only stone chips and flakes were recovered (all organic material had disappeared), the creative integration of micro-topographic studies, refitting and use-wear analysis allowed to obtain a fuller picture of the people at the very end of the Ice Age. That picture was like an ethnographic snapshot of the past—Van Noten literally named it 'palaeoethnography'. However, explicit ethnographic parallels were avoided throughout, despite Van Noten's profession in an ethnographic museum, his position in the department of anthropology in Leuven, his travels in the Kalahari, and his archaeological fieldwork in Bantu Africa. Even if the excavated evidence was scant, ethnography was not explicitly invoked as a source of information. Though the experience with the !Kung might have coloured Van Noten's vision, he ostensibly avoided the crafting of such analogies: 'Ethnographic parallels can only be drawn when there is historical continuity between what you excavate and what is still alive; contemporary Bushmen can only tell you about earlier Bushmen but that's it,' he told his students in

*Leuven—myself being one of them. In the late twentieth century, ethnographic analogies had to be applied with much more rigour than most visitors to the colonial exhibition ever assumed.*¹

Let us retreat from this narrative. This is not a history of the Tervuren museum after all, no matter how much such a study deserves to be written (but see Luwel 1960; Wynants 1997).² The history of Belgian institutions, and even of Belgian science in general, is not the scope of this book. If I present such sketches from the museum's centennial existence, it is because, more than any other place I know, it has been an exemplary locus of the changing discourses on primitives and primates that I wish to study.³ The late nineteenth-century belief in contemporary savages as living ancestors, the growing interest for great apes during the Interbellum, the quest in postwar primatology for the extant species which gave the best hominid model, the recent critique of such modelling exercise, and the distrust of straightforward ethnographic parallels in modern archaeology, all these ideas, at some point, were articulated in Tervuren. From the spectacle of Congolese savagery to the silent measurement of dusty bones, these ideas centre on one theme: what are legitimate sources for enhancing our image of prehistoric humanity?

Archaeological and palaeoanthropological data are almost by definition imperfect. Even if depositional conditions were always mint, excavations exhaustive, and hominid fossils abundant, in such empirical Walhalla the data would still be insufficient to answer all our questions. Archaeologists and palaeoanthropologists wish to reconstruct past behaviour—in the widest sense of the term: from early hominid locomotion to Upper Palaeolithic rituals—and it is a truism to state that behaviour itself does not fossilize. There is, therefore, an important epistemological gap between the formal object of the disciplines (reconstruction of behaviour) and their material object (material culture, fossils). As a corollary, archaeologi-

1 More details to this historical sketch can be found in a range of publications. On Tervuren and the colonial exhibition, see Luwel (1960), Van Reybrouck (1997a) and especially Wynants (1997: 119–128, 157–161). The illustrations in the latter are particularly evocative: my description of the arrival of the Congolese in Antwerp is based on a press drawing by Gailliard (Wynants 1997: 121); the portrayal of life during the exhibition used historical photographs (Wynants 1997: 122–5); and the quotes came from press articles published in *Le National* (7 and 12 June 1897) and *La Gazette* (9 June 1897), copies of which were generously given by Maurits Wynants whom I want to thank for several discussions and unlimited exchange of information. The weather report at the day of the arrival was provided by the climatological service of the Royal Meteorological Institute of Uccle in Belgium (letter 24 November 1999; ref. Clim/Fax/99377); a description of the thunderstorm and inundations appeared in *Ciel et Terre* (1897: 255). Darwin's famous line about the Fuegians comes from the *Descent of Man* (1871: II, 404). On the discovery of the bonobo, see Coolidge (1984) and Van den Audenaerde (1984), and of course the original publications (Schwarz 1929 and Coolidge 1933). More recent studies on the Tervuren collection of bonobos include Zihlman and Cramer (1978) and McHenry (1984), see also Haraway (1989: 340). On bonobos in captivity and behavioural studies on them, see De Bois and Van Puijenbroeck (1993). The excavations of Meer were published by Van Noten (1978) and are described by Renfrew and Bahn (1991: 280–1).

2 References in the text always give the original date of publication. Quotes are generally from first editions or original publications, unless otherwise stated between brackets in the list of references.

3 To avoid the ponderous term 'nonhuman primates', monkeys and apes will often be designated simply as 'primates' as opposed to 'humans'. Of course, this is not to deny that humans are also primates, but a succinct terminology which is easily understood seems far more preferable than a cumbersome taxonomically and politically correct jargon.

cal and palaeoanthropological interpretations are always underdetermined by the data; a leap must be made to cross the gap between what we see and what we say. It is here that the use of external sources becomes of paramount importance. Analogies, parallels, comparisons, models, and metaphors are all devices which purport to bridge the gap. In general, the procedure is to invoke a more familiar instance to clarify one which is less familiar to us. In historical sciences like archaeology, palaeoanthropology and also geology, this comes mostly down to looking at the present where processes are still at work to explain the past where we find only mute patterns, the result of such processes. Analogies thus explain the unknown in terms of the known, the past patterns in terms of the present processes. Analogies are powerful instruments to disentangle complex problems, but risk to obfuscate the original issue when it is substituted by the more familiar instance. Whereas a certain amount of analogical reasoning is useful, too much reliance on it can blur the issue.

Two external sources have been of crucial importance in the study of prehistoric behaviour: contemporary ‘primitives’ and extant primates. The former were intensively tapped in the Victorian anthropology of the second half of the nineteenth century and continue to be so, though in a very modified form, in modern Anglo-American ethnoarchaeology; the latter became more prominent in North-American primatology after the Second World War. The contributions of primitives and primates to the image of human prehistory and evolution are so essential that it is surprising no one has undertaken a serious study of them. Indeed, the influential American primatologist Linda M. Fedigan once remarked that ‘a discussion of the proper use of analogy in natural and social science might be useful in the context of human evolution theories’ (1986: 45) but regretted that it was not yet done. Beyond this weak statement, nothing of the sort has been formulated. The present work attempts to fill that void.

By analysing key texts from the history of anthropology, archaeology, and primatology and subjecting them to a logical reading grid, this work seeks to study the way analogies from primitives and primates have been used during the last two centuries to elucidate human prehistory. More precisely, I want to investigate which sources were selected, how the analogies were made, how they were defended and how they were strengthened. My interest is first and foremost in the structure of analogies, less in their outcome. By drawing on recent insights from inductive logic, philosophy of science and cognitive psychology, I shall attempt to analyse the argumentative mechanisms underlying both primate models and ethnographic parallels. I focus on three debates: the one surrounding the comparative method of Victorian anthropology, the one dealing with modern ethnoarchaeology and the one on the postwar use of behavioural primate models. The emphasis is on Anglo-American science where these issues have been elaborately debated. This is not to belittle the importance of the continental tradition; but its often autonomous and different development simply falls beyond the range of this work.

Perhaps I should specify further what this work will not embark upon. Obviously, my aim is not to find ‘correct analogies’ for deducing how early hominids behaved, how Neanderthals looked like and what Magdalenian rock art

meant. Though not entirely detached from current debates in archaeology and palaeoanthropology, such substantial themes are beyond the scope of this study. In a sense, my interest has been more in prehistorians and primatologists than in prehistory and primates. However, if I took some distance towards ‘real’ archaeology in order to study its history, the present work does not belong to ‘classical’ history of science either. Whereas proper historians of science, generally speaking, study scientific ideas by contextualizing them in their particular social, institutional, biographical, political and ideological settings, I have often been consciously decontextualizing historical statements in order to make their logical structure comparable across the ages. Though I have situated historical debates within their respective polemical contexts (which is a *sine qua non* for any understanding), ultimately my aim was to compare their argumentative skeleton with debates from other periods. The frame of reference relied upon here was thus not one of strict synchronous contexts but of diachronic structures: rather than studying thin time-slices of the history of science, my temporal horizon was broader (and, by definition, coarser) than many historians would permit. More a conceptual than a contextual historiography, my work is somehow related to the discipline of history of ideas in the sense it was inaugurated by Arthur O. Lovejoy in the 1930s (Lovejoy 1936; 1938). Indeed, the long-term perspective and the search for transhistorical recurrences echo the approach Lovejoy c.s. followed (Wilson 1987; Oakley 1987; Kelley 1990). Yet mine is still not a case-study in the history of ideas. Its traditional stress on relatively immutable unit-ideas which are transmitted from generation to generation contrasts with my focus on the bricolage of analogical reasoning where time and again inferences have to be invented and defended. This, then, as will become clear, is a history of arguments. In touch with (but distinct from) the sciences it studies, from history of science and from history of ideas, it attempts to unravel the procedures by which inferences and ideas are crafted.⁴

The perspective taken should be further delineated. My uneasy relation with traditional history of science stems from a debate which has been going on in the history of human evolution studies over the last decade (Van Reybrouck 2000, *in press*). Apart from studies undertaken by classical historians of science (cf. Bowler 1986; Theunissen 1989), the 1990s have witnessed the emergence of an alternative form of historiography written by scholars who are more involved with current debates in archaeology and palaeoanthropology (Landau 1991; Stoczkowski 1994). The differences between these histories are rather impressive. Whereas the former focus on the detached study of short-term scientific contexts, the latter are often committed to demonstrating how pre-scientific, mythical and narrative forms from the past still affect contemporary science. It is a matter of synchronous contexts against diachronic structures, of historicist methodology against structuralist ontology, of neutral history of science against critical history of

⁴ Margaret Hodgen who studied Tylor’s doctrine of survivals (1937) coined the term ‘history of scientific method’, a term I feel sympathy with.

archaeology and anthropology.⁵ The whole debate pivots around the notion of continuity. For ‘structuralist’ historians of science, continuity is the rule rather than the exception; often similarity between two historical statements is enough to infer continuity. According to them, scientific ideas must be seen as outcrops of underlying, fairly immutable, long-term structures. For ‘classical’ historians of science, each historical context is unique and must be understood in its own terms; continuity with former periods will only be accepted if there is sufficient evidence for direct transmission by intellectual borrowing, personal tutorship, institutional affinity and educational background. The confrontation on this interesting theme reached a tentative apex during the *Does history matter?* conference held in Leiden in January 1997 (Corbey and Roebroeks 2000, in press; Bowler 2000, in press; Stoczkowski 2000, in press; Theunissen 2000, in press). The mutual criticisms were far from mild: classical historians reproached the structuralist approach of undue century-hopping, of ripping sources out of their context, of reading modern meaning into ancient texts; ‘structuralist’ historians decried the temporal myopia, the short-term bigotry and contextual dogmatism of traditional history of science. Rather than choosing sides for one of the camps, I believe that there is some truth in both critiques. Studying long-term structures does indeed require more methodological rigour than most structuralist historians have been willing to admit, but this should not imply, as classical historians urge, that the project must be abandoned as a whole in favour of only short-term perspectives. The historicist method and structuralist question can be combined; in a recent paper on the meaning of bipedalism, Wiktor Stoczkowski (1995) has shown how an interest for continuity can be fruitfully reconciled with an interest for discontinuity. My own position, therefore, stands midway between both paradigms. Whereas my interest is in long-term structures, in continuity and in critical history, I have not relied upon structuralist method because it runs too easily in the trap of selective reading and because it often sees continuity when there is just similarity. Instead, my study of three debates (Victorian comparative method, modern ethnoarchaeology, and postwar primate models) proceeds by a fairly traditional historicist method of detailing the respective polemical contexts, after which I move beyond the intricacies of the short term, to indicate and explain some of the similarities and differences between the debates as long-term continuities, recurrences or discontinuities.

Another axis along which I should position myself concerns the issue of presentism versus historicism. More obsolete than the continuity-discontinuity debate, it still figures prominently in the history of archaeology (Fahnestock 1984; McVicar 1984; Pinsky 1989; Van Reybrouck 1995; Gustafsson 1998; 1999; Jensen 1998). In 1968, George Stocking coined and defended the notion of historicism

5 It cannot be incidental that nearly all proponents of the continuity thesis share a background in structuralist anthropology. Wiktor Stoczkowski came from Polish ethnography and prehistory and used Lévi-Straussian methods in his analyses of hominization scenarios; Misia Landau, a palaeoanthropologist by training, drew on Russian formalism (especially Propp’s method of studying folk tales) and to a lesser extent on French structuralism (like Greimas’ narratology; cf. Lewin 1997: 30–46). Others like Perper and Schrire (1977), Latour and Strum (1986) and Sahlins (1996) have intellectual affinities with structuralist anthropology.

as the detached study of the disciplinary past, the study of the past for the sake of the past that is typically undertaken by professional historians of science. He argued that in the long run, such neutral attitude might be more beneficial to contemporary issues in the field than an overtly presentist agenda of studying the past for the sake of the present, a Whig historiography which is often characteristic for practising scientists-turned-historians (Stocking 1968a; Di Brizio 1995). Ever since, presentism has become somewhat of an insult for practising scholars with an interest in their discipline's past. It should not be forgotten, however, that at the time Stocking wrote his historicist defence, anthropology witnessed some of the excesses of presentist historiography (Harris 1968).⁶ Neo-evolutionist scholars were re-establishing links with the founding fathers of anthropology from the second half of the nineteenth century in ways which, in retrospect, seemed far more rhetorical than historiographical. Stocking was right to repudiate the unabashed judgmental attitude of their histories, their tendency to divide the past in winners and losers, their wish to decide who was right and who was wrong. Yet the historicist alternative, on the other hand, turned out to be something of a positivist chimera: a respectable ideal of intellectual neutrality which could never be reached because of the theory-ladenness of observation. But should it be reached? Perhaps not necessarily. It has been the great insight of hermeneutic philosophy and philosophy of science that being tied to your own time is not necessarily an inhibition to understanding the past but also a condition of possibility. Gadamer stressed the constitutive role of prejudices and Kuhn argued that a paradigm provided a vision of the world. Present suppositions are often likened to a pair of spectacles which colour and deform one's vision—an unfortunate metaphor which apparently forgets that glasses make the world visible, albeit it in specific ways. In recent years, therefore, people have urged for a critical history of science, a history which does not deny its links with the present, which hopes to contribute to contemporary debates not by applauding the present state of knowledge (like in the Whiggish history) but by questioning and deconstructing it, by showing the contingencies of what we now know, by indicating that our present understanding is as much influenced by intellectual legacy as it is by empirical adequacy (Pinsky and Wylie 1989; Veit 1998). Such presentism is not history *for* the present but history *of* the present, and sometimes even *against* it.⁷

I feel sympathy with such a programme as it navigates between unattainable objectivist pretensions and all too easy subjectivist agendas. Moreover, this 'critical presentism' also enabled me to find a place in the institutional context I have been working in. As this Ph.D. research formed part of a larger project where geologists, palaeontologists and 'real' archaeologists focused on the Palaeolithic

6 In archaeology at that time, the work of Glyn Daniel (1967) was not free from presentist underpinnings either. It described the history of archaeology as a ladder that had been ascended through time, as a series of steps which cumulated in the present state of understanding (cf. Trigger 1985; Richard 1993; Veit 1998). He thus used modern theories as a yardstick with which to assess the relative merits of previous interpretations.

7 In biology such a role for history has been already long-time acknowledged. Key participants in the debate are often respected historians of their own discipline. The historiographical and biological work of authors like E. Mayr and S.J. Gould are truly integrated.

occupation of Europe, I was constantly reminded of the relevance disciplinary history could and should have on current themes.⁸ As an archaeologist working on the history of his discipline, I had to satisfy myself with a somewhat ambiguous position: whereas I thought (and still think) that an awareness of the field's past is essential to any participation in contemporary debates, I did not regard it the historian's task to supply ready-made answers to the practising scientist. If archaeology is like a game of football, I do not see myself as one of the players on the field, nor do I long to be the trainer who shouts from the sideline. The role of neutral referee or fanatic supporter appeal even less to me. If there is one person I feel affinity with, it must be the commentator during an international match who tries to be objective, while barely hiding his sympathies. Neither the field or the sideline, nor the dugout or the stands but the press booth is the place for the critical presentist.

Though the present work discusses themes as diverse as Faroer oil lamps, baboon dentition and postmodernism in archaeology, this variety is bound together by a comprehensive theme I came upon during my research. Having worked on ethnographic analogy in nineteenth-century Neanderthal research (Van Reybrouck 1994a) and being acquainted with some of the debates in ethnoarchaeology, a repeated pattern struck me as I worked my way through the primatological literature. *Primate models as they developed in the second half of the twentieth century seemed often more closely akin to ethnographic parallels in Victorian anthropology than to modern ethnoarchaeological analogies which explicitly moved beyond the confines of the nineteenth-century comparative method.* Indeed, chimpanzees sometimes appeared to be today's Tasmanians. Both were used as living stand-ins for the remote human past, as flesh and blood versions of the fossils excavated. My working hypothesis was that after the Second World War, the discourse on primitives would have been displaced to the nonhuman primates so that modern primate models came to echo the presuppositions of an earlier, anthropological tradition, whereas ethnoarchaeology was precisely seeking procedures to circumvent the straightforward projections of this older anthropology.

This very simple hypothesis has rarely been hinted at before me, and even then only in a cursory way. Donna Haraway's monumental *Primate Visions* mentions the '1950s relocation of discourse about primitivity onto monkeys and apes' but remains silent about the impact on analogical arguments (1989: 229). In 1993, Sarah Lyon read a paper at the annual meeting of the American Anthropological Association on the historical shift from an ethnographic other to a primate other, arguing that 'the ethnographic other never dies because all the other concepts are kept implicitly alive in the primate other' (1993: 385). The paper, however, was never published. In her outstanding review of baboon studies, Susan Sperling (1991: 11) wrote that 'monkeys and apes were used explicitly as exemplars of earlier stages of human evolution. The ubiquitous primate ancestral group now occupied a position like that of "tribal societies" in the evolutionary schemas of

8 This was the Palaeolithic Pioneer Project 'Changing Views of Ice Age Foragers' at Leiden University which received funding from the Netherlands Organization for Scientific Research.

nineteenth-century anthropologists.' It is the most explicit reference I have found to the working hypothesis I had coined. The present work, then, can be seen as one long argument elaborating this point.

Chimpanzees are today's Tasmanians. This is the central theme of the present work and we might as well stop here, were it not that 'history is that impossible thing: the attempt to give an account, with incomplete knowledge, of actions themselves undertaken with incomplete knowledge' (Graham Swift, in his novel *Waterland*). Simple and attractive though my working hypothesis seemed at first, it dawned on me that historical reality was far more complex every time I got stuck in some text which did not respond to my expectations. Initially I had the idea of presenting an ideal-typical scheme of the analogies per debate (similarity-based, formal analogy for the nineteenth-century comparative method; causality-based, relational analogy for the twentieth-century ethnoarchaeology; and similarity-based, formal analogy for modern primate models devoid of the nineteenth-century hierarchy), but time and again I had to admit that half a century of discussion cannot be resumed under one such generalizing description. In particular, I grew aware of the danger of stereotyping an entire controversy on the basis of its most extreme utterances. Morgan and Sollas are indeed epitomes of evolutionist reasoning, but the picture is much more subtle. Binford and Whitelaw do indeed pinnacle ethnoarchaeology's search for alternatives, but others were less outspoken. Tanner and Zihlman do exemplify the postwar quest for a primate model, but not everybody has been so fanatical. For this reason I decided to describe each debate first of all in its concrete polemical context rather than in abstract, ideal-typical terms; only then could cross-disciplinary comparisons be made. Logical simplicity thus made place for historical subtlety, which is a nice way of saying that my argument became more muddled. A working hypothesis is no more than this, something to work with. The moments I had to nuance my hypothesis, I felt comforted by what Franz Boas once said on new ideas: 'The invention is not difficult. Difficult is the retention and further development' (1888: 638).

What does the further development of the argument look like? Chapter 1 provides the relevant theoretical and methodological instruments. Drawing upon insights from inductive logic, philosophy of science and cognitive psychology, it presents an idiom to talk about analogies as well as a method for studying them. It argues that analogies are central to the practice of science, that they are not right or wrong but more or less valid, and that there exist devices for strengthening them. Somewhat technical in tone, the chapter introduces concepts and principles which will be used throughout the subsequent chapters.

Chapter 2 takes us to Victorian anthropology and its use of ethnographic parallels. The chapter places the development of its famous comparative method against the background of the changing polemical contexts (such as the three-age system, the antiquity of man, the degenerationist critique and the late-evolutionist fragmentation) and notes how the analogies were increasingly based on a narrow definition of similarity and became projections.

Chapter 3 shows how after a relative silence in the first half of the twentieth century, ethnographic analogy emerged again as part of the New Archaeology. It investigates how the resultant ethnoarchaeology attempted to move beyond the comparative method and how the notion of analogy was thoroughly discussed. The chapter also deals with ethnoarchaeology's fall from grace in recent theoretical archaeology.

Chapter 4 is a study of the debate on primate modelling as it developed in postwar primatology. It details why monkeys and apes were only recently drawn on as sources for human behavioural evolution and how, from the 1960s onwards, scholars have been favouring baboons, social carnivores, geladas, chimps, and bonobos as 'best models' for studying early hominids. Analysing the arguments put forward, it also explains the crisis of such referential modelling and the alternatives it has given rise to recently.

Whereas the three preceding chapters can be read in their own right, Chapter 5 confronts them with each other. It demonstrates how ethnoarchaeological analogies and primate models, although contemporaneous, differ profoundly. It further investigates the structural resemblance between the analogical reasoning of post-war primatology and Victorian anthropology.

The conclusion argues that the logical resemblance between the comparative method and primate modelling is not so much due to a direct transfer of methods and procedures, but the result of a discursive continuity in the representation of tribal societies and primate groups between the nineteenth and the twentieth century. As a corollary, primitives have been replaced by primates as entities for immediate and wholesale projections towards the distant human past.

Analogies

Analogy in science

Nearly anyone who has ever studied physics will remember how the behaviour of gases was presented with the example of permanently moving billiard balls. Just like the red and white balls on a cloth, gas molecules were said to move randomly through a finite space and the smaller this space was, the more frequent the collisions became, resulting in higher pressure. Pascal's law was nothing else but a game of pool. Examples like this permeate the practice of science and their visualizing power makes them powerful devices in the transfer of otherwise abstract forms of knowledge. The fact that many people still remember aspects of the kinetic theory of gases, even long after they have given up studying physics, is perhaps the best illustration of the pervasive nature of such models.

Apart from their didactic and mnemonic potential, models, metaphors and analogies also serve as important heuristic devices in the discovery and elaboration of scientific theories.¹ The history of science amply provides us with examples such as Kekulé's well-worn account of the discovery of the circular structure of benzene after his dream of a snake biting its tail, Bohr's discovery of the atomic structure by analogy with the solar system, and Darwin's discovery of natural selection inspired by Malthus' theory on human population growth (Holyoak and Thagard 1995: 209). Here, the role of models is not merely confined to the transfer and reproduction of scientific knowledge but plays an integral part in the production of it. Cognitive psychologists have experimentally shown that people will more easily solve an abstract problem if they can invoke an external analogous model. In a classical experiment, students were asked to solve a medical problem of how one can apply radiation to a malevolent tumour inside someone's stomach without the bundle of rays destroying the stomach tissue beyond repair. Only ten percent of the students came up with the right solution: split up the rays and attack the tumour from different angles. In a next stage of the experiment, respond-

1 There are as many definitions of analogy, model, and metaphor as there are students of them (Hesse 1966; Leatherdale 1974; Sapir 1977; Lakoff and Johnson 1980; Holyoak and Thagard 1995). Theories regarding metaphor have even been called 'the Balkan of literary theory' (Draaisma 1995: 19). This diversity has at least the advantage that one is free to develop a personal vocabulary. The terms model and analogy are defined in the subsequent section with that title; the term metaphor won't figure prominently in the present work (not because it has little relevance, but because it was rarely used in the historical debates I want to describe) although the philosophical discussion between authors like Ricoeur and Derrida on its constructive or deconstructive role is closely associated with the debate on analogy.

ents were asked to read a story about a general who wanted his army to attack a town that was surrounded by vast minefields. The general decided to divide his army into smaller contingents which could cross the minefields and attack the town from its different access roads. When students were told to use this story to solve the tumour treatment problem, 75 percent succeeded in finding the right solution. Clearly, the military analogue had helped them to solve the medical problem (Holland et al. 1986: 289-96; Holyoak and Thagard 1995: 110-6).

Once the importance of analogies in reasoning is realized, the question shifts from whether or not analogies are essential, that is, inextricably bound up with the process of scientific growth. Whereas many would argue that models and analogies are only temporarily useful devices which can be rejected once the theory is formulated, others hold that they are permanently required in the practice of science (Wilson 1964; Hesse 1966: 1-56; Leatherdale 1974: 39-90; Stepan 1986). Freud elaborated an interesting intermediate position by saying that whereas particular metaphors might become redundant, each stage of scientific development still requires the invention of new metaphors (Draaisma 1995: 16). If metaphors, models and analogies can be seen as tools, are they tools of construction or tools of maintenance? Are they disposable tools or durable implements? Or is it still possible, as some would argue, to perform certain tasks with bare hands? These questions are far from being resolved philosophically, but from the history of science it is undeniable that models and analogies have been ubiquitous throughout very many scientific disciplines at a level far beyond their didactic purposes. '*Raisonner par analogie c'est construire la pensée*', as the French logician Maurice Dorolle wrote more than half a century ago (1949: 178).

Analogy in archaeology

Archaeology forms no exception. Basic concepts from the disciplinary past like 'the ladder of inference', 'the archaeological record', and 'material culture as text' are in fact metaphorical constructs. To speak about the past and the study of it inevitably requires the imaginative use of external devices. A recent book like Steven Mithen's *The Prehistory of the Mind* (1996) makes ample use of analogies, models, and metaphors. Dealing with an abstract notion like 'the human mind' the author drew on several metaphors to visualize his ideas to the reader. As such, the mind was likened to a sponge, a computer, and even a Swiss army knife (to illustrate its presumed modular build-up).² Yet beyond this tutorial value, much seems to indicate that metaphorical reasoning was instrumental in the development of Mithen's theory. This is especially true of his key comparison between the evolution of the human mind and the development of the gothic cathedral. According to Mithen, the mind had originally started like a Norman church with a nave of general intelligence. Later, multiple chapels of specialized intelligences were added, and finally these modular intelligences were integrated and connected like the chapels of a gothic cathedral. Much seems to indicate that this architectural history of the medieval church was more than just an illustration of the evolutionary development of human cognition. Mithen himself admitted that when he started reading on evolutionary

2 There is a long-standing history of visualizing the human mind, and in particular memory, by means of metaphorical associations. See Draaisma (1995) for an excellent study.

psychology he was reminded of the excavations of an Italian church in which he had participated as a student. It is therefore unlikely that his whole theory was in place before the architectural metaphor was drawn. On the contrary, the perception of the mind as a cathedral must have helped him to develop his ideas in an otherwise fairly abstract research domain. The architectural analogy might be unusual in archaeology, the use of analogies as such is certainly not.

Judging from the wealth of publications, the most acknowledged form of analogical reasoning in archaeology is without doubt the use of ethnographic analogy. The tenor of debate about it sounds much like the one described above. While most archaeologists would rapidly agree on the historical role analogy has played in the formative stages of the discipline, especially in the recognition of stone tools (Orme 1981: 2-21), the opinions are much more divided when it comes to an appreciation of today's application of ethnographic analogies. Richard Gould's characterization of it as 'an idea whose time has *gone*' (Gould 1980: x, original italics) is diametrically opposed, ostensibly on the face of it, to Ian Hodder's assertion that 'all archaeology is based on analogy' (Hodder 1982a: 9). Similarly, when Leroi-Gourhan believed that analogies might have been useful for nineteenth century scholars, he strongly urged modern archaeologists to abandon '*ce folklore scientifique*' (Leroi-Gourhan 1964: 151). Clearly, what is at stake here is the same question whether analogies are provisional or permanent in science. From a historical perspective, however, there is no need to settle this tenacious dispute; but observing the pervasiveness of analogical arguments throughout the history of the discipline, even implicitly in the works of critics such as Leroi-Gourhan and Gould (Wylie 1982), gives credit to the theory of a central importance of analogies. If all archaeology is somehow analogical in practice, if not in principle, this strongly calls for a detailed historical study of the use of analogies in reconstructing the past.

The role of primate models in the construction of our image of the past has been much less appreciated by archaeologists. This is due to a number of factors. In the first place, analogies based on primatological observations are a relatively recent phenomenon. Whereas ethnographic 'savages' were already since the Renaissance interpreted as living relics representative of the European past, substantial primate models only began to be formulated after the Second World War (see the introductions of Chapters 2 and 4). Secondly, analogies based on primatological observations have almost exclusively been applied to the specific field of early hominid studies in Africa; a more general archaeological appreciation beyond these spatial and temporal confines was thus prevented. Compare this with the world-wide application of ethnographic parallels for periods ranging from the Upper Palaeolithic to postmedieval times. Thirdly, students of early hominids and their material remains have often followed a different theoretical course related to the philosophy of the natural sciences than the more hermeneutic tack often steered nowadays by archaeologists working on more recent periods. Based on an erroneous equation of primatology with sociobiology, a general 'culturalist' reluctance to consider aspects of the biology of fully modern humans, has only increased this polarization between Palaeolithic and more recent archaeologies (Loy

and Peters 1991). Fourthly, the majority of these models have been constructed by primatologists and palaeoanthropologists, not by archaeologists. In fact, the desire to shed light on human nature and its origins was a main incentive to the rise of primatology, and although nowadays this anthropocentric question is no longer the only motivation, there is hardly a grant proposal or a textbook in primatology which does not try to draw implications about humans, modern and fossil ones alike, by comparing them with primates. And fifthly, primatologists and palaeoanthropologists talk about primate *models*; archaeologists about ethnographic *analogies*. This different terminology has also contributed to the absence of primate models in archaeological discussions on analogy. Apart from a short article by Foley (1992), primate models and forager analogies have thus far not been treated in the same conceptual terms. Of course, this is not to deny the vast ontological differences between humans and primates, but methodologically both primate models and ethnographic parallels rely on similar arguments by analogy.

Despite this disciplinary gap between the ethnographic analogy and the primate model, both archaeologists and primatologists make claims about the past derived from observations of currently living systems. There is no final reason for excluding primate models from a historical study of analogy, as long as we make clear what we understand by ‘model’ and ‘analogy’.

Models and analogies

The terminological discrepancy between primate models and ethnographic analogies is interesting and needs further clarification. Rather than trying to define the concepts in strictly logical terms (a point which, moreover, has never been adequately settled), it is more interesting to look at what they have meant in archaeology and primatology itself. The terms should be understood by their structural opposition in both disciplines, not by any *in vitro* definition of their semantic signifieds.

In archaeology, the term analogy came in use during the 1960s, particularly with Ascher’s landmark ‘Analogy in archaeological interpretation’ (Ascher 1961) and Binford’s ‘Smudge pits and hide smoking: the use of analogy in archaeological reasoning’ (Binford 1967). Of course, many ethnographic parallels had been drawn before, but it was only in the vocabulary of the early processual archaeology that ‘analogy’ became the term to indicate this practice and it has remained so ever since. The concept of ‘model’, on the other hand, surfaced a bit later in the early 1970s with the infiltration of processual thought and systemic geography in British archaeology.³ Two highly influential volumes of the time had the word in their title, *Models in Archaeology* (Clarke 1972a) and *The Explanation of Culture Change: Models in Prehistory* (Renfrew 1973a). From the very beginning, model had a broader meaning than analogy. Clarke (1972b, 1) stated that ‘models are pieces of machinery that relate observations to theoretical ideas.’ When he discussed ‘the morass of debate about the proper and improper use of historical and

3 British archaeologists borrowed the notion of model from the New Geography. As an inductive approach (cf. infra) it was quite different from the hypothetico-deductive and deductive-nomological obsessions in American archaeology at the time.

ethnographic “parallels” in archaeological interpretation’ he called it ‘merely a particular setting of the universal debate about the proper and improper use of models in general’ (Clarke 1972b: 40). Of course, there are very many functions and definitions of models (cf. Apostel 1961), but in archaeology the term generally designated some form of formalized explanatory framework for a set of phenomena, which could be established by means of an ethnographic analogy, but also through a replicative experiment, a computer simulation, a lawlike generalization, a statistical prediction or any other ‘model’ (Clarke 1972b). ‘Model’ was something that stood between a specific interpretation and a general theory or a law; it often consisted of mathematical equations, flow charts and feedback mechanisms. It referred both to the explanation as to the source on which this explanation was built.

Both terms thus originated in the context of processual archaeology and since this has been exposed to various forms of criticisms, it should be no surprise that the popularity of the terms has varied accordingly. For instance, in recent years contextual and post-processual archaeologists dispelled the notion of ‘model’ altogether as it was felt to be too heavily laden with connotations of formal, mechanistic, positivist, systemic explanation—which conflicted with their view on interpretation and their definition of humanity. The term ‘analogy’, on the other hand, was already criticized in the late 1970s by certain processual authors. Gould repeatedly urged to move ‘beyond analogy’ (1978b; 1980; Gould and Watson 1982) and Schiffer (1978: 234) wanted ‘to dispense entirely with the word *analogy*’, believing that ‘*model* or *hypothesis* will usually provide a better fit’ (original emphases). Nevertheless, these were only attempts to reject one particular form of analogy, i.e. projective analogy, in favour of another, more sophisticated one. ‘The reaction against analogy’ (Wylie 1985) has therefore never effaced the popularity of the term in archaeology. On the contrary, today the very word ‘analogy’ still functions as a shorthand for ‘ethnographic analogy’, even if very recently certain post-processualists prefer to talk about metaphorical associations than analogical links (Tilley 1999; Holtorf 2000).

In biology, the term analogy dates at least back to the nineteenth-century comparative anatomy of Richard Owen who distinguished it from homology. The human hand and the wings of a bat were said to be homologous because of their *similarity of structure*; but the wings of the bat and the wings of a fly were called analogous because of their *similarity of function*. Later, after the evolutionary turn, homologies were translated as ‘the response of the same organ, inherited from a common ancestor, to different selective pressures’ and analogies as ‘the response by different organs to the same selective pressures’ (Cain 1976: 26). This vocabulary was not restricted to anatomy only. With the rise of ethology in the 1920s, it started to be applied to animal behaviour when field biologists like Niko Tinbergen and Konrad Lorenz began to treat fixed patterns of behaviour in the same terms as organs (Atz 1970). According to them, behavioural patterns such as the stickleback’s courtship display could be studied like the wings of the bat and were thus treated as either homologous or analogous. Popularizing the results of the ethology, authors like Lorenz and Desmond Morris were eager to establish further analogies between animal and human behaviour. Lorenz’ *On Aggression* (1966) explained human violence by reference to wolf behaviour, Morris’ *The Naked*

Ape (1967a) explained human sexuality by reference to chimpanzee behaviour. Yet their often impressionistic method became so much criticized that the very notion of analogy came to carry a very negative stigma. S.A. Barnett's review of *On Aggression* was called 'On the hazards of analogies' (1968). Barnett regretted that 'so much talent should have been misapplied' and found the analogical method 'essentially anti-rational': 'This method should be repudiated by all scholars—indeed, by all responsible people' (83). The leading primatologist Zuckerman (1981: 387-97) warned that these sorts of analogies only led to anthropomorphism and this was probably the last thing a young discipline like primatology wanted. He wrote: 'I shall be surprised if analogical writing does not in the end bring discredit on the field of study now called primatology' (Zuckerman 1981: 392). In the early 1970s, the term 'model' started to be preferred to indicate the inferences that were drawn from baboons, chimps and bonobos; while the term 'analogy' was abandoned or only applied to a remote source like the gelada baboon where there was no danger of anthropomorphism. To many primatologists, the emotive value of 'model' was that of scientific, sound, reliable; that of 'analogy' impressionistic, vague, and speculative.

This particular terminological history explains why primatologists prefer to talk about 'baboon models' and 'chimpanzee models'. Like in archaeology, 'model' indicates both the source in the present world as well as the explanation resulting from that source.

Analogy as a process

A history of terms is not a history of concepts. The terminological discrepancy between archaeologists and primatologists can be easily overcome and reconciled. In fact, this was already done by some of the practising scientists themselves. On one of the rare occasions in which an archaeologist debated the use of external sources with primatologists, Richard Potts could be heard to speak of 'analogies, or species-specific models' (1987: 34). According to him the structural equivalence between both was evident. Jeanne Sept, an archaeologist who studied chimpanzee nesting, equally noted that primatologists who worked at 'modeling' were comparable to archaeologists who 'have long debated the appropriate use of analogy in prehistoric reconstruction' (1992: 204). Indeed, both terms are closely related. Whereas in primatology the preference for the word 'model' refers to the starting point (the model as source) and the end point (the model as explanation) of the argument, the notion of analogy which prevails in archaeology relates to the inferential process between these two extremes. 'The relation between the model and the observations modelled,' David Clarke (1972b: 2) wrote, 'may in general be said to be one of analogy.'

In this work, I will consider *analogy as the process by which the model is put to work*. In logical terms, the analogy is the 'argument' linking the two senses of the word model, i.e. from 'premises' to 'conclusion'. The model (as source) from the present world—no matter whether we talk about an Aboriginal stone-knapper or a troop of savannah baboons—can only yield statements about the past (the model as explanation) through an argument by analogy. Analogy is the bridge between both.

As the main question of this work concerns the inferential structure of arguments rather than their inferential extremes, I prefer to use the term *analogy*.

Analogy has moreover received widespread attention from scholars in inductive logic, philosophy of science and cognitive psychology. Aristotle already discussed analogy as a literary trope, J.S. Mill laid the foundations of inductive logic in the nineteenth century (after the long-standing dominance of Baconian deductive logic), but it was only in the second half of the twentieth century that philosophers and logicians seriously addressed the issue of scientific analogy. Mary Hesse's *Models and Analogies in Science* (1966) was a landmark which influenced all other writings on the topic. It specified the relations within the analogy and stressed the importance of causality. Leatherdale (1974) introduced the notion of manifest and imported analogues. The work of Salmon (1963), Copi (1972), Barry and Soccio (1988) and Freeman (1988) detailed the criteria for appraising analogies. Whereas all these authors treated analogy as a finished mental product, in recent years cognitive psychologists have started to investigate the process of analogical reasoning. By means of large-scale experiments, they study how human reasoning actually occurs when analogies are drawn. Inspired by Schön's (1963) stimulating work on the displacement of concepts and Lakoff and Johnson's (1980) famous essay on metaphor, authors like Holland, Nisbett, Gentner, Hofstadter and especially Holyoak and Thagard have greatly enhanced our understanding of analogy (Holyoak and Thagard 1995; 1997). Holyoak and Thagard's *Mental Leaps: Analogy in Creative Thought* (1995) forms the putative apex of this innovative research field. An excursion into the disciplines of inductive logic, philosophy of science and cognitive psychology is therefore required if we want to develop a precise conceptual language for talking about analogies.

The structure of analogy

Analogical inferences are perhaps the most commonly used form of reasoning and occur as much in day-to-day life as in scientific practice (Holyoak and Thagard 1995). If I go to a specific pub on Friday night because I remember that the previous times I was there the atmosphere was quite nice, the beer rather good and the people friendly, I am basically relying on an analogical argument which is not very different from the billiard ball model for gases. Indeed, despite their enormous internal differences, 'all analogical arguments have the same general structure or pattern' (Copi 1972: 353).

My expectation that I will have another good time in this pub is based on a number of similarities with my former visits: it is again Friday evening, the interior is the same, the same friends are present, the landlord hasn't changed, and the beer is supplied by the same brewery. Under these conditions, I had a nice evening last time; therefore, all things being equal, I might have a good time now. The same holds true with the billiard balls. Because of the observed similarities between gas molecules and billiard balls (spherical volumes, elasticity, motion and impact in bounded space, etc.) and because billiard balls are known to collide

more frequently when space is reduced, therefore, the gas molecules will have a higher impact rate if the reservoir is smaller.

I am aware of the fact that there is quite a distinction between pub visits and gas molecules. In the one case, we speak about two specific instances from the same domain; in the other about two distinct domains that are mapped onto each other. Leatherdale (1974) has introduced the distinction between ‘manifest analogue’ and ‘imported analogue’, whereby the former relates to proximate sources of inspiration from the same domain and the latter to remote sources of inspiration from very different domains. The distance between the analogues is much larger in the gas case than it is in the pub case; and the regularity is lawlike for the one and statistical for the other. However, these differences do not affect the structure of the argument (Copi 1972; Holyoak and Thagard 1995). This becomes clear if we schematize both examples. The pub case can be translated as follows:

This time and last time I went into this pub, the landlord, the brewery and the interior were the same;

Last time I was here I had a good time;

therefore, this time I will probably have another good time.

Similarly, the billiard ball example becomes:

Gas molecules and billiard balls have the same impact and motion;

Billiard balls collide more frequently in a reduced space;

Therefore, gas molecules will probably collide more frequently in a reduced space.

In both examples, we predict new similarities on the basis of observed similarities. If A and B are instances with properties x, y and z, then the above examples can be formulated schematically (cf. W. Salmon 1963: 70; Copi 1972: 353; Kondakow 1978: 28; M. Salmon 1982: 61):

A and B have properties x and y;

B has also property z;

therefore, A has probably also property z.

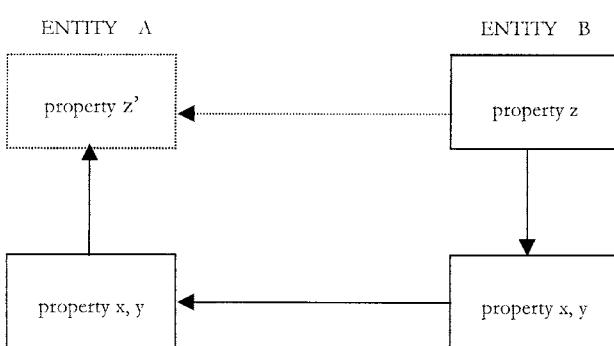


Figure 1. A diagram for visualizing the logical structure of analogical arguments

This simple scheme forms the core of any form of analogical reasoning. It can also be visualized in a diagram (figure 1). Let the right side of the figure be the more familiar pair of the analogy and the left side the less known entity and let the observed resemblances serve as the bottom line on which the predicted similarity rests, then we easily recognize the logical structure of every analogy.

The most enigmatic word in the scheme above is the word ‘probably’. In a discussion of analogy the issues of truth and certainty cannot be passed over in silence.

Truth and validity

Whenever studying analogies, the logical distinction between truth and validity is essential. This is a theme where logicians have been very firm: if analogies are arguments, it follows that *analogies can never be true or false*. Truth is an attribute of a conclusion or a premise and refers to the material content; validity is an attribute of an argument and refers to its formal structure. Thus, conclusion and premises can be true or false, but arguments are said to be valid or invalid (Hodges 1977: 53-60). To say that ‘this or that analogy is true’ comes down to a logical absurdity. The reason why I stress this is because all too often confusion has arisen by claims that a true analogy or a best model have been found. An analogy is neither true nor false, it is at best valid and inspiring.

The relation between truth and validity is a fairly complex one. In strict logical terms, a valid argument does not necessarily entail a true conclusion and, inversely, an invalid argument may contain true premises and conclusions (Van De Putte 1982: 6-8). Consider the following well-known examples.

I fit into my pyjamas;
My pyjamas fit into my suitcase;
Therefore I fit into my suitcase.

Formally, this argument is valid but its conclusion is false (because the characteristic of physical volume which is essential to the pyjamas when we speak of ‘fitting into’ is different in the first and second premise). Yet when I state that:

All humans are mortal;
Socrates is mortal;
therefore, Socrates is a human.

the premises and the conclusion are true, but the argument is invalid because there are possible situations in which the premises are all true but not the conclusion. This would for example be the case if Socrates was the name of my dog. (Note that although truth and validity are two distinct qualities, the validity of the argument could only be ascertained by looking for true or false counterexamples.) The above argument would become valid if we interchange the second premise and the conclusion:

All humans are mortal;
Socrates is a human;
therefore, Socrates is mortal.

This is the classical example of a logical syllogism which respects the formal laws of validity. The argument is valid because not one case can be imagined in which all the premises are true and the conclusion false. Therefore, a valid argument is like ‘a mechanism’ which transfers the truth of the premises to the conclusion (Van De Putte 1982: 8). Neither the material truth of the premises nor the formal validity of the argument does on its own guarantee the truth of the conclusion. If both premises are true and the argument is valid, the conclusion will be equally true. Both are as important.

But to return to analogies. In order to judge whether certain analogical conclusions are true, one does not only need to investigate whether the premises are true (i.e. whether the right ethnographic case or primate species is invoked), but also whether the argument by analogy is valid. All too often, discussions on analogical reasoning remain confined to a discussion of the premises, without considering the argument itself. To use an architectural metaphor, much attention has been spent to the furniture, but far less to the construction of the building itself.

What is this validity? All the examples I have used thus far derive from deductive logic which is the strongest branch of logic, because the truth of the premises is transferred by necessity and with absolute certainty to the conclusion. However, all logicians agree that the argument by analogy is an inductive form of reasoning and therefore less strong than the perfect syllogism (Dorolle 1949: 170-2; W. Salmon 1963: 70-3; Copi 1972: 351-68).⁴ After all, the truth of an analogical conclusion does not derive from a general rule such as in deductive reasoning, but from a parallel case. Analogical arguments have rightly been called ‘ampliative’ because their conclusions tell more than the premises, i.e. they expand rather than deduce the truth from the premises (Wylie 1985). Analogies are generally less strong than deductive arguments, but this does not mean that all arguments by analogy are equally acceptable. Hodges says that ‘arguments can be good without being valid. We may call an argument *rational* if its premises provide good reason for believing the conclusion, even if the reason is not absolutely decisive’ (Hodges 1977: 59, original italics).

When it comes to the truth claim of inductive conclusions, a relative *probability* can be assessed. The logician Copi (1972: 38) is clear about this:

Although no argument by analogy is ever valid, in the sense of having its conclusion follow from its premisses with logical necessity, some are more cogent than others. Analogical arguments may be appraised as establishing their conclusions as more or less probable.

In a similar spirit, the French philosopher Dorolle speaks of ‘les degrés de valeur d’une conclusion analogique’ and ‘un coefficient appréciable d'affirmation’ (Dorolle 1949: 168, 172) whereas the Russian logician Kondakow uses the idea of ‘Wahrscheinlichkeitsgrad von Analogieschlüssen’ (Kondakow 1978: 28). Holyoak

⁴ An analogical conclusion may be as sound as one reached by deduction but the problem lies in the difficulty of determining its truth.

and Thagard, two cognitive psychologists who have published extensively on analogy, regard it as ‘a source of plausible conjectures, not guaranteed conclusions’ (Holyoak and Thagard 1995: 30).

In sum, in inductive arguments truth and validity are transformed into probability and plausibility. If we want to investigate analogies, we shall have to deal with relative plausibility rather than absolute validity. For logicians, the lack of optimal strength may be disappointing; for historians, however, this makes analogies all the more interesting. Indeed, such inductive challenges have provoked a wide-ranging diversity of solutions through time (as will be clear from the following chapters). If induction is sometimes called ‘the scandal of philosophy’, historians wring their hands about such scandal.

Entities and relations

Scandals and excitement notwithstanding, we first need to elaborate a precise vocabulary for talking about analogies in the study of prehistory. My attempt to apply concepts from logic to archaeological analogies is far from original; this received considerable attention during the early 1980s (cf. Hodder 1982a: 16–24; M. Salmon 1982: 57–83; Wylie 1982; 1985). Especially Alison Wylie’s 1985 paper ‘The reaction against analogy’ still remains, after nearly fifteen years, the best treatment of the problem. Yet what distinguishes my approach from previous ones, is my insistence on the distinction between truth and validity and my reliance on insights from cognitive psychology. Unlike former workers in this field, I also use this logical framework primarily as a tool for historical analysis rather than as a device for methodological improvement.⁵ And my intention to treat primate models in similar terms as ethnographic analogies sets it apart from the hitherto exclusive attention to ethnographic parallels.

Let us start with a historical example. When early modern antiquarians invoked descriptions of the use of stone tools amongst non-European natives in order to explain the function of what had previously been considered ‘thunderbolts’, ethnography served the same function as the billiard balls and the previous pub experiences described above, i.e. as sources of familiar knowledge for explaining an unfamiliar observation. Since the unknown is interpreted in terms of the known, all analogies can be divided in two parts or analogues, with the *source* being the more familiar side from which predictions are made and the *target* as the part of the analogy which is under study (Holyoak and Thagard 1995: 2). These terms are to be used in the widest sense. The source can be anything from a specific tool type in Papua New Guinea to a cross-species correlation between body size and range of territory in monkeys and apes; the target may range from a functional attribution of a tool to an entire scenario of hominid evolution. Synonyms for this twin concepts include alternatives like ‘model and referent’, ‘base and target’, ‘source and subject’, ‘vehicle and tenor’, and ‘discontinuous and continuous term’ (Hesse 1966; Leatherdale 1974; Sapir 1977; Moore 1996).

5 See also Ravn (1993) who applied Wylie’s logical framework to the history of analogy in Danish prehistoric studies.

Another distinction should be made. The antiquarians started from an observed similarity between the form of present and prehistoric stone tools, and formulated a predicted similarity in terms of function. If you have only physical remains, these observed similarities always concern material aspects on the basis of which non-material aspects are inferred (Stoczkowski 1992). The form of a stone tool is a *material* aspect, its function is *non-material*. These concepts are related to the Binfordian terms ‘statics’ and ‘dynamics’ which are, however, too specifically archaeological to be useful for our purpose.⁶ ‘Material’ and ‘non-material’ are to be preferred over statics and dynamics, although often the idea of observed and predicted similarity will suffice.

The last pair of concepts is less problematic to define. Antiquarians disposed of three categories of observation: the form of the prehistoric tool, the form of the ethnographic tool, and the function of the ethnographic tool. What was to be explained was the fourth category: the function of the prehistoric tool. In an archaeological analogy an inference is always drawn from a set of *observable* phenomena in the present about a *non-observable* aspect of the past (Gifford-Gonzalez 1991). The source side of the analogy evidently belongs to the realm of contemporary observations, but it has been rightly stated that ‘all the observational statements generated by the archaeologist [are] contemporary facts’ (Binford 1981: 22). Therefore, what we will term *observables* does not only encompass the material and non-material entities of the source, but also the material entity of the target, i.e. the archaeological and fossil record. In practice, however, Palaeolithic archaeologists and palaeoanthropologists will often stress the observables of the record, while primatologists and ethnoarchaeologists will work more confidently on the observable source side.

The six entities of an archaeological analogy, (source and target, materials and non-materials, observables and non-observables) can be easily summarized in a scheme (figure 2). On their own, these entities do not constitute an analogy as long as the relations between them remain unconsidered. Following Mary Hesse (1966: 59), two sorts of relations can be discerned in our diagram: *horizontal relations of similarity* and *vertical relations of causality*. The former refer to the resemblances between source and target in terms of identities or differences; the latter to the relations between the material and non-material entities. Hesse (1966: 8) distinguishes between three forms of similarity relations: ‘positive analogy’ for the observed similarity between source and target, ‘negative analogy’ for the observed

6 Since Binford (1981), we are well accustomed with the terms ‘statics’ and ‘dynamics’ whereby the former refer to material traces and the latter to behavioural correlates, comparable to the footprint and the bear. Despite being attractively succinct, these terms have the disadvantage of connoting some form of causality, statics were thought to be caused by dynamics. While this is true in fields such as zooarchaeology where observed patterns such as cut-marks need to be interpreted in terms of inferred processes, the distinction is too specific for the rest of archaeology. In reaction to Binford’s view of material culture as a static reflection of a dynamic living system, several authors have stressed the active and dynamic qualities of material culture. Talking about ‘statics’ and ‘dynamics’ is also inadequate when we want to include the field of palaeoanthropology. If we find an increase in brain size during the early Pleistocene, the observation is surely material but not static: rather than being unidirectionally caused by an immaterial, dynamic process, this pattern is itself causative for a number of immaterial effects such as cognition and language. The material entity is here the causing bear and the created footprint is archaeologically lost.

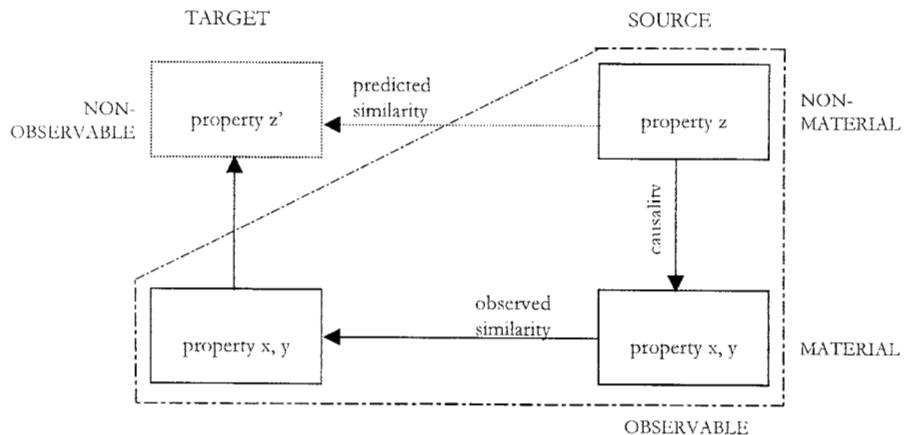


Figure 2. The entities and relations in an archaeological analogy

dissimilarity, and ‘neutral analogy’ for properties whose similarity is unknown. The latter category is the most interesting one, because it enables us to predict further, unobserved similarities. Hesse’s distinction is a useful one because it reminds us that analogical reasoning always proceeds from observed similarities to predicted similarities *in the light of manifest differences*. I will return to this issue later on.

Similarities can be situated at different levels: between attributes, between propositions, and between systems (Holyoak and Thagard 1995: 101-37). Similarity of attributes consist of one-to-one statements of the sort ‘*a* is *b*’ or ‘*a*::*b*’ (for instance, ‘the smooth cutting edge of a prehistoric axe is like the one found on a Papuan axe’). Similarity of propositions detail relations between attributes. This is the classical proportional analogy which has the form ‘*a* is to *b*, as *c* is to *d*’ or ‘*a*:*b*::*c*:*d*’ (for instance, ‘the polish on the prehistoric tool relates to its specific function, like the polish on the Papuan tool to cutting trees’). Similarity of systems detail relations between propositions so that one can say ‘*A* is to *B*, as *C* is to *D*’ or ‘*A*:*B*::*C*:*D*’ (for instance, ‘the overall characteristics of the prehistoric tool such as polish, form, weight, raw material suggests a function of an axe, just like all characteristics of the Papuan tool were related to that of cutting trees’). Whereas similarities between attributes are the ones that are most rapidly noted, similarities between relations and systems are often more interesting. For example, the formal resemblance between a prehistoric and a Papua New Guinean axe may be apparent, but the principle that an axe requires this or that specific form is much more interesting to reason from. A good analogy specifies in the first place resemblance of relation (between attributes, between propositions), rather than form (of the attributes).

The second set of relations concern the causality between the material and non-material entities. Observing the similarities and dissimilarities between source and target will obviously not yield interesting predictions if the observed and predicted similarity are causally unrelated. For example, if I would infer that the atmosphere in the pub will be as convivial as it was last week because I am wearing the

same pair of shoes as last Friday, the argument will not be very strong as there is no causal link between my shoes and the pub's ambiance. Similarly, if an antiquarian would reason that a prehistoric tool had this or that function because its colour is the same to that of ethnographic specimens, his conclusion will be doubtful. Hesse was the first to stress that the observed and predicted similarity need to be causally related and this is only possible if the similarity is one of relations rather than attributes. Causality, however, has a dubious meaning when studying prehistory as it is generally acknowledged that only some patterns of the past are causally explicable, whereas many forms of complex human behaviour defy any such interpretation. Rather than repeating a long discussion on where to draw the extent of causally explicable phenomena in the past, it is important to underline that Hesse uses the term causality in its strict logical sense. How students of prehistory have dealt with this notion of causality will be discussed in subsequent chapters.

An analogy in the study of prehistory can thus be defined as an argument whereby a prediction is made about an unobserved, nonmaterial aspect of the target on the basis of observed similarity (in the light of observed dissimilarity) between material aspects of the source and target which are to some degree causally related to their nonmaterial aspects.

An ideal case

Granted that no analogy is ever absolutely valid, under what conditions do we reach maximum plausibility? If the empirical content of the entities is true, the plausibility of an analogy hinges upon the quality of its vertical relation of causality. Ideally, this relation should be unambiguous in the present and uniformitarian to expand it to the past. 'If A then B' should be 'if and only if A then always B'.

In order to be *unambiguous*, the relation between material and non-material entities in the present source will need to be causal not only in Hesse's logical sense but also in practice. This causality requires to be exclusive: 'if and only if'. If we want to infer the function of a prehistoric stone tool by analogy, we will have to be sure that the axe-like tool we observe in ethnographic contexts could not have been anything else but an axe. Form and function need to be unambiguously linked. If we find that some axes have entirely different forms or that some axe-like tools do not serve as axes at all, our argument will be seriously flawed. This frequent source of confusion is recognized as the problem of equifinality—the same effect can be caused by a number of different processes—and is antithetical to the desired unambiguity.

Related to the problem of equifinality is the fallacy of affirming the consequent. It goes like this: 'a tool for cutting trees has the form of an axe; we found an axe-like tool; so it was used to cut trees.' The structure of it is 'if A then B, we have B, therefore A'. Why is this wrong? A straightforward example makes this clear: all black animals are raven, I have found a black animal, therefore it is a raven. Wrong of course, because it could be a bat, a fly or a panther. The fallacy of affirming the consequent is a classical error against the syllogism: its premises may be right, but since there are cases where the conclusion is wrong, the argument is

invalid. The problem is that it bases its conclusion on the consequent (the minor term) rather than the antecedent (the major term). In archaeology, the danger of committing this fallacy is very real: since we have only material remains from the past, and since these are often the static result of an unknown antecedent, we affirm the wrong entity. An axe-like form? It must be an axe, we reason, forgetting that an axe-like form may have very different functions.

Establishing a causal, unambiguous link in the present is not enough if we want to make reliable predictions about the past. The link will also require to be *uniformitarian*, i.e. we need to know whether the processes we observe today are similar to the ones in the past: ‘then always B’. We might find that the form of an axe today is causally and unambiguously determined by its function, the functional interpretation of a prehistoric tool will be much stronger if this link holds across time. Following Lyell’s original use of the term, uniformitarianism has provoked an extensive but unresolved discussion in archaeology. Just as with the concept of causality, uniformitarianism became another processual pet notion (Binford 1978b: 12; 1981: 27; Gould and Watson 1982) which was generally thought to be quite reliable in mechanistic realms such as taphonomy and zooarchaeology but much more problematic when it comes to complex forms of human behaviour. To speak of uniformitarian assumption presupposes that one knows how causes behave through time, and this is very often the question at stake.

If causal unambiguity and uniformitarianism can be firmly established, it is easy to see how the analogical source and target have become two distinct expressions of an underlying lawlike regularity. This is where the predictive power of an analogy turns into a mere illustrative function, because when all causal links in the past and present are known, the analogy has been replaced by a theory. In this context Mary Hesse speaks of formal or posttheoretic analogies which are ‘different interpretations of the same formal theory’ as opposed to material analogies or ‘pretheoretic analogies between observables [...] which enable predictions to be made’ (Hesse: 1966: 68). In the ideal case, the analogy is so strong and so independent from its specific source, that we have reached an explanatory theory.

Clearly, this is only very rarely the case. Both causal and uniformitarian assumptions have only a limited field of application which is restricted to the most mechanistic realms where inferential confidence can be high (Gifford-Gonzalez 1991). Once we move beyond these realms, however, and try to make wider, behavioural inferences, causal let alone unambiguous and uniformitarian relations become often hard to establish. As a corollary, many, if not most, analogies are far removed from this ideal case. Are there other criteria we can use to evaluate the strength of these analogies? How are we to distinguish between weak and strong analogies? If the item under study is too complex for a mechanist analogy, how can we further improve the analogy?

Strengthening the analogy

Since the argument by analogy is a form of inductive reasoning, the validity and truth can never be as absolutely warranted as in deduction. One of the most commonly held opinions says that the best way to assess an analogy is by *testing*

(Holyoak and Thagard 1995: 30). Again, we need to stress that not the analogy is being tested here, but the prediction based on that analogy. Testing is not a procedure for establishing the validity of an argument *a priori* but for determining the truth of the conclusion *a posteriori*. As the emphasis here is on the form rather than the content of analogies, I will only briefly glance at the problem of testing archaeological analogies; the ways to improve the validity of the argument will be discussed next.

No matter how shaky the foundations of an analogy are, through rigorous testing, so the popular argument goes, one can escape the confines set by the source. Relegating the analogy to the in positivist's eyes inferior context of discovery, testing is supposed to belong to the more secure context of justification. It is through *a posteriori* testing that the speculative prediction of the analogy can be turned into reliable knowledge (or else rejected). The problems with this hypothetico-deductive approach are manifold since testing is 'far from being an objective confrontation of "ideas" with "facts", it is a complex, thoroughly inductive process of continual adjustment between the theoretical frameworks... and the facts' (Wylie 1985: 88). With regard to analogy, some specific problems emerge. Firstly, there is never a total independence of the context of analogy. Because 'prior probabilities' (M. Salmon 1982: 78) are attached to certain analogies (not every analogy is considered), it follows that what finally goes through testing is always a selected set of hypotheses. Secondly, only rarely do the test implications follow with logical necessity from the predictions and very often they only amount to indicating further similarities between source and target rather than to independently testing the prediction (Hodder 1982a: 21-2). Thirdly, even if we can formulate an independent and causal set of test implications, we will only be able to do so by means of another analogy with the present world from where we extract another link between an observable and non-observable entity (Stoczkowski 1992). Ironically, then, it seems that to escape the use of analogies through testing, we ultimately are destined to fall back on them.

The testing of analogical predictions will play a profound role in our analysis of primate and forager models, but we first need to investigate the criteria for assessing the plausibility of analogies (figure 3). This shifts the emphasis from truth to validity, from content to form, and from *a posteriori* to *a priori*. Here, the standard logical literature on analogy provides us with a useful conceptual apparatus that allows us to distinguish a number of criteria (W. Salmon 1963: 70-3; Copi 1972: 358-62; M. Salmon 1984: 65-6; Barry and Soccio 1988: 188-95; Freeman 1988: 322-4). Cognitive psychology provides very useful additional information.

(1) A first yardstick concerns the *number of similarities* argued from. The more resemblances between source and target, the greater the strength of the argument. If the observed similarity is weak, the predicted similarity will be weak as well. When comparing prehistoric objects with ethnographically known stone tools, the number of resemblances in material, form, fabrication and use-wear will determine the plausibility of the inference. Adding similarities to the premises certainly strengthens the argument but it would be erroneous to think that if all aspects in an analogy are similar a 'perfect analogy' is found (Wylie 1982: 393;

Fischer 1970: 247-8). Copi (1972: 358) has rightly said that ‘it should not be thought that there is any simple numerical ratio between the number of points of resemblance asserted in the premises and the probability of the conclusion’. For one thing, an argument by analogy is always based on the recognition of similarities in the light of existing differences. If there were no dissimilarity, the relation of analogy would become a relation of identity.

(2) Since all analogies contain dissimilarities, the extent of this ‘negative analogy’ should be realized. Clearly, the *amount of dissimilarity* will have an impact on the argument’s cogency. This principle is no more than the inverse of the first, but in order to fully appreciate an analogy, the dissimilarities should be made explicit. Continuing the lithic example, we may have to note that the colour, the weight, and the raw material of our prehistoric stone tool is different from the ethnographic one. However, such differences are not by definition to be avoided. On the contrary, to have a truly interesting analogy, a certain amount of dissimilarity is needed. The billiard balls model only works *because* of the distance, not *despite* it. Analogies re-format a less familiar domain with the structure of a more familiar domain, so the latter should at least be somehow different to produce new insights; if not the analogy becomes a tautology and loses all its heuristic potential. Compare it with the use of metaphor in poetry. If the poet says ‘November is a nineteenth-century’⁷ the similarities of melancholy, darkness, nostalgia are immediately clear, but the evocative power of the image only works *because* of the difference, because a month cannot be century. Or compare it with rhyming verses: a melodious rhyme occurs when two verses end on similar sounds; yet if the sounds would be exactly the same, the rhyme would be a dull repetition. A good rhyme thus requires a good balance between similar sounds and different sounds. Of course, science is no poetry and arguments not rhymes. Yet the lyrical examples are instructive: they show that the more remote the source analogue is, the more innovative its contribution to the problem can be. (Just like the realm of poetry itself, as a remote source, can be instructive to think about science.) Cognitive psychologists found that ‘the most interesting examples of retrieved source analogs are those in which a useful source is found in a domain that seems far removed from that of the target’ (Holland et al. 1986: 309). Holyoak and Thagard agree:

A complete isomorphism has nothing to be filled in, leaving no possibility for creative leaps. Incompleteness may well weaken the confidence in the overall mapping, but it also provides the opportunity for using the source to generate a plausible (but fallible) inference about the target’ (Holyoak and Thagard 1995: 30).

Dissimilarity does not only form a problem of analogy but also a possibility.

(3) Arguing from similarities and dissimilarities between source and target will be seriously improved when the *relevance* of each of these is considered. In the aforementioned pub example, the observation that I am wearing the same shoes is indeed a point of similarity but not a very relevant one when it comes to predicting the ambiance inside. In the case of our prehistoric stone tool, resemblances in form, fabrication, and especially use-wear with the ethnographic example are

7 ‘November is een negentiende eeuw’ is a line of the Dutch poet Guillaume van der Graft.

all extremely relevant because they are causally related to their function in the present. On the other hand, the similarity in raw material would be less if it appeared that other types of stone like flint or serpentine could perform equally well. When it comes to dissimilarities, here also some are relevant and others are not—this is what the distinction between structure-preserving and structure-violating comes down to. The difference in colour is presumably not a very crucial one when it comes to functional attribution yet a considerable difference in weight is a dissimilarity which might weaken the argument. In order to interpret the function of the tool, then, we will need to weigh the relevance of the observed similarities and dissimilarities.

As a consequence, enumerating similarities is no longer enough to make a compelling analogy. ‘*Le nombre des ressemblances ne suffit donc pas*,’ observes Dorolle (1949: 149), ‘*On est toujours ramené au même problème: d'où vient leur signification?*’ He then goes on to distinguish between *ressemblances significatives* and *ressemblances trompeuses* (158). Along with Dorolle, nearly all other logicians have stressed the importance of relevance. Kondakow (1978: 27) urged for ‘*wesentlich gemeinsame Merkmale*’, whereas Copi (1972: 360) is very explicit about the value of relevant similarities:

The question of relevance is all important. An argument based on a single relevant analogy connected with a single instance will be more cogent than one which points out a dozen irrelevant points of resemblance between its conclusion's instance and over a score of instances enumerated in its premisses. (original emphases)

Cognitive psychologists, too, stressed that the relevance of similarity was more important than the amount of it. Holland, Holyoak, Nisbett and Thagard wrote: ‘Even in the ideal case not all elements of the source situation need to be mapped. The most critical elements are those that were causally relevant to the achieved solution’ (Holland et al. 1986: 297). Considerations of relevance thus move the discussion from strictly formal analogies⁸ towards more relational analogies (Wylie 1985). The former are based on similarity of attributes, the latter on similarity of propositions (relations between attributes) and systems (relations between propositions). Although the distinction between both types is a gradual one and a continuum can be outlined between their extremes, relational analogies focus more emphatically on the principles of connection between the material and non-material entities of both source and target. Since in practice the non-material entity of the target is rarely known (in fact, this is precisely what is at stake), we try to detect principles governing the source side such as the required form of contemporary stone axes and project these principles onto the past. In a relational analogy, the argument shifts from the horizontal relations of similarity to the vertical relations of causality, bringing the principles of correlation close to Hesse’s logical causality. Copi (1972: 361) expands upon this:

⁸ Formal analogy in Wylie’s sense (1985) is quite distinct from the same term with Hesse (1966). For Wylie, formal analogies, opposed to relational analogies, are the ones that do not consider relevance but simply enumerate similarities. Hesse distinguishes formal from material analogies; the former are posttheoretical and only illustrative, the latter are pre-theoretical and heuristic.

One property or circumstance is relevant to another, for purposes of analogical argument, if the first affects the second, that is, if it has a causal or determining effect on the other.

The factor of relevance is to be explained in terms of causality. In an argument by analogy, the relevant analogies are those which deal with causally related properties or circumstances. (original italics)

The two following criteria immediately hinge upon the notion of relevance.

(4) The greater the *number of source contexts* in the premises, the stronger the argument. If a certain pattern between material and non-material entities is observed in a great number of instances, the correlation appears to be truly genuine instead of incidental. If not one Friday night in the pub was fine, but a dozen of them, this gives me more confidence about the expected ambiance. Similarly, if my correlation in the present world between form and function of a stone tool holds not only for one group of Papua New Guinean flintknappers, but instead for a great number of them, my argument will be improved. Expanding the source of the analogy, therefore, raises the inferential confidence.

(5) The larger the *variety of source contexts*, the more cogent the analogy will be. Here we are not only increasing the quantity of the instances but also their quality. A Friday night is likely to be nice if also Tuesday mornings, Thursday afternoons and Saturday evenings are known for their good ambiance. The functional interpretation of a prehistoric stone tool will be heightened if the correlation between form and function not only occurs among Papua New Guinean tribes, but also among Australian aborigines, !Kung bushmen, and Efe pygmies. This variety of source contexts suggests that the pattern is likely to be more than incidental, but somehow general. Broadening the source to include a great variety of contexts does improve the quality of the inference because ‘the more dissimilar the instances mentioned in its premises, the stronger is the argument’ (Copi 1972: 359).

(6) A final criterion considers the *weight of the conclusion* with regard to the premises. There should be a balance between the initial premises and the eventual conclusion. If we conclude from a handful of formal similarities between our tools that the prehistoric object was used as an axe in a matrilineal, horticultural and totemic prehistoric society, clearly the conclusions outweigh the premises. Similarly, if I inferred from the given circumstances that my night in the pub will consist of six pints of beer, four good jokes and one proper conversation with a friend, my analogical inference will be more easily rejected. Freeman (1988: 322) has called this the ‘inverse variation principle’, which comes down to the statement ‘the stronger the conclusion, the weaker the argument’. And, Popperians would add, the easier the falsification. According to falsificationism, one’s conclusions should be as precise as possible so that falsification might be easier. This is not in contradiction to the inverse variation principle since both call for accurate, precise inferences rather than vague generalizations on the basis of constrained premises. Attributes should be carefully transferred rather than holistically projected towards the target.

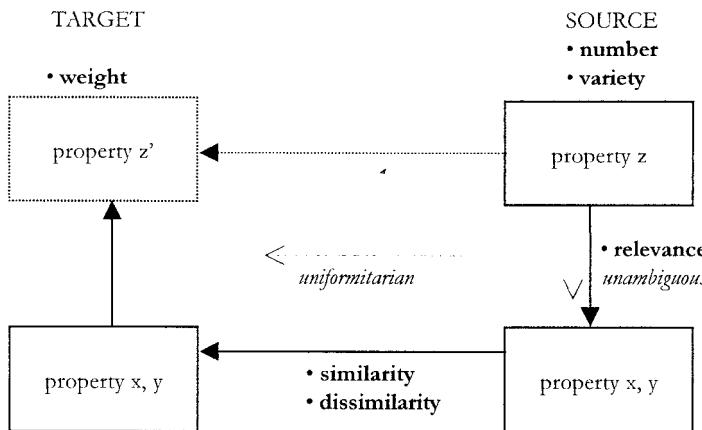


Figure 3. Strengthening the analogy. In the ideal case (here in italics) the causal relation is unambiguous within the source and uniformitarian with the target. Mostly, however, improving the analogy will consist of increasing the number and variety of sources, improving the amount of similarity, considering causal relevance and keeping the balance between the weight of the conclusion and the premises.

These six criteria help to evaluate the relative validity of any analogy and can also be used to improve the strength of existing analogies. In the following chapters I shall not be concerned with improving particular analogies, nor do I wish to solve a specific empirical question. Nevertheless, the above taxonomy of strength criteria will prove extremely helpful in dissecting the use of analogies in the past.

In sum, I have discussed two fundamentally different ways of strengthening an analogy. *Testing*, though highly problematic, serves to assess the truth claim of the analogical conclusion. *Improving* the internal structure of the analogy by means of a number of appraisal criteria, enhances the relative validity of the argument. With this, we have specified the entities, relations and strength criteria of analogy. We now need to study the distinct stages of analogical reasoning.

The practice of analogy

Analogies have thus far been described as existing, self-contained, complete, finished arguments while neglecting that these arguments are the result of a process of construction. If we want to study the history of analogical arguments, it is imperative to take one step backwards to see how analogies are actually assembled, fabricated and put to work. The question shifts then from ‘how does an analogy work?’ to ‘how is an analogy created?’ Donald Schön, who was one of the first to study the actual manufacturing of analogies and metaphors, speaks in this context of their ‘developmental process’ or ‘life cycle’ (Schön 1979: 260) and indeed, it is possible to formulate an ideal-typical sequence of steps taken in the construction of an analogical argument. This can be called the *analogical algorithm*.

The analogical algorithm

If a female chimpanzee from the Taï forest in Ivory Coast wants to crack a *Panda oleosa* nut, which has one of the hardest shells of all African nuts, she chooses first a robust stone hammer and an adequate anvil. When she starts to pound the nut, the efficiency of her tools is put to the test and she might choose to change them. The ultimate aim, of course, is to open the nut and eat it. In this simple example, a technological sequence can be observed involving the definition of a problem (how to open the nut), the choice of a method (selection of hammer and anvil), the implementation of the method (pounding), the evaluation of the method (keeping or rejecting the hammer), and ultimately the desired solution (opening the nut). In fact, when archaeologists construct an analogy, they are going through similar conceptual stages. Earlier in this chapter, analogies have already been described as cognitive tools. It should not come as a surprise, therefore, that the five steps in the technological algorithm are essentially the same in an analogical algorithm: problem identification, source selection, source implementation, evaluation, and conclusion. Each of these steps entail a number of questions. Interestingly, cognitive psychologists have suggested a very similar fivefold division; they believe that three major constraints on analogy (similarity, structure and purpose) are operative in each of these steps, though with differing emphases.⁹

(1) *Identification of a problem* is closely related with the purpose of the analogy.¹⁰ In the first step of an analogical algorithm, the archaeological or palaeoanthropological problem at stake is defined. This stage of the algorithm is entirely situated at the left side (target side) of the analogy and asks what we want to know about the past on the basis of the given evidence. Cognitive psychologists name this stage ‘encoding the target’ or ‘transforming the target problem’ (Holland et al. 1986: 307). Although not always rendered explicit, propositions at this stage can be seen as answers to the following questions: What is the target, which spatio-temporal segment of the past do we study? What is the observable and material part of the target, which evidence do we have? What is the non-observable and non-material part of the target, what do we want to know? In order to solve this identified problem, a whole arsenal of archaeological methods will be invoked. Those who support the use of analogical arguments will have to indicate their ethnographic or primatological sources.

(2) With the *selection of a source analogue*—psychologists use the same term or speak of ‘retrieval of a source analog’ (Holland et al. 1986: 309; Holyoak and Thagard 1995: 115)—we move to the right side of the analogy. Questions underlying this step are: What is the source of the analogy? Do we base ourselves on a single source or on a number of source instances? How is the choice of the source legitimized? Which arguments do we invoke to justify the choice of the source and

9 It came as a surprise to see that cognitive psychologists had been working with a similar analogical algorithm as the one suggested here. Their ideas have certainly helped to refine my scheme, though it was developed independently.

10 Cognitive psychologists disagree as to the importance of purpose. Holyoak and Thagard take a more pragmatic perspective in that they stress the constraint of purpose, especially in problem definition, source selection and evaluation. Others, however, most notably Gentner, think that analogy should be understood in purely syntactic reasons. The issue is far from clear and dates back, in fact, to Peirce’s distinction between semantics, syntax and pragmatics (see Gentner 1983; Spellman and Holyoak 1996; Holyoak and Thagard 1997 and Gentner and Markman 1997).

perhaps to reject other potential source instances? This is one of the most crucial steps in the argument and, considering our focus on primitives and primates, much attention will be given to it.

Since psychologists are mostly concerned with how an individual reasons, they have often stressed the role of memory in source retrieval: ‘The selection of source analogs highlights one of the most intriguing qualities of human memory,’ Holyoak and Thagard wrote (1995: 116). ‘For an autonomous problem solver the most difficult step in the use of analogy is likely to be the retrieval of a plausible source’ (Holland et al. 1986: 312). Through an ingenious experiment, they have demonstrated that the most typical criterion for source selection during the retrieval phase is surface similarity, irrespective of its structural properties (Holyoak and Thagard 1995: 107-8). When asked for advice about American military intervention in a fictitious war situation, participants to the experiment (who were undergraduate students in political science) let their decision-making depend on totally irrelevant parallels to known wars (source analogues). If the situation described showed some superficial resemblances to World War II (refugees, for example, were said to have fled in boxcars of trains), respondents favoured intervention. If the identical situation showed minor likenesses to the Vietnam War (refugees were now said to have fled in small boats, for instance), respondents advised against intervention. Surface similarity thus constrained to a large extent the selection of a source analogue: ‘Salient properties, even those that are functionally irrelevant to a solution to the target problem, may affect the solution plan indirectly by influencing the selection of a source analog’ (Holland et al. 1986: 313). This entails a danger:

The danger, of course, is that superficial cues will favor the retrieval of particular source analogs over others that would be equally useful and would suggest alternative courses of action. Analogy, like all forms of induction, cannot be divorced from risk. (Holland et al. 1986: 314)

Now, this risky aspect will of course be minimized when we consider analogical reasoning not just in terms of individual, autonomous thought, but of collective, disciplinary endeavours. But even then, the attraction of surface similarity will not always fade away.

(3) The *implementation of the source* comes down to the logical structure of analogies as defined above. Cognitive psychologists speak of ‘mapping’ and ‘transfer’ (Holland et al. 1986: 307; Holyoak and Thagard 1995: 121-30). If analogical inferences proceed from the recognition of similarity between source and target (mapping) and attribute further properties to the target on the basis of their presence in the source (transfer), then we should be able to identify the following three questions: What is the observed similarity between source and target? Which additional properties are found in the source only? And what is the predicted similarity?

Experiments have shown that in this mapping and transfer the role of surface similarity is inferior to that of structural similarity. Once a source analogue is known, ‘people are extremely good at finding sensible mappings even when the

analogs are far from isomorphic' (Holyoak and Thagard 1995: 129). However, there are also 'limitations on human mapping ability' (130), especially when both analogues have very different internal structures or when people lack expertise to move beyond details of a specific field in order to see its deeper, more abstract structures.

(4) The *evaluation of the analogy* is the place where the initial purpose come back into the picture. Holyoak and Thagard argue, however, that this step that is often passed in silence:

Once two situations have been mapped and the source has been used to generate inferences about the target, one might suppose that the job is done. In fact, this is the stage at which analogies become most dangerous. If the mapping seems coherent and the inferences are not obviously implausible, then people are likely to feel that they truly understand the target domain. But there is an ever-present threat: despite what the analogy suggests, the target domain simply may not behave in a way that parallels the source domain. In other words, despite its intuitive appeal, the inferences generated by an analogy can turn out to be wrong or seriously incomplete. (Holyoak and Thagard 1995: 131)

They suggest, therefore, to give more consideration to the issue of testing, though they admit that this is not always easy. Indeed, it is important to consider questions like: Is the conclusion testable (verifiable)? If so, is it tested? And does the test confirm the prediction? Is the conclusion falsifiable? If so, is it falsified? And does the falsification refute the prediction?

However, this cannot be enough. What Holyoak and Thagard seem to miss is that such testing only assesses the truth claim (probability) of an inference, not its validity (plausibility). This is where inductive logic still has more to say. The plausibility of the analogy can be appraised by means of the six strength criteria detailed above: What is the number of similarities argued from? What is the number of dissimilarities? What is the relevance given to each of these (dis)similarities? What is the number of source contexts considered? What is the variety of source contexts? And what is the weight of the conclusion relative to the premises? Evaluating an analogy is more than testing its propositions, it consists of checking its strengths.

(5) *Conclusion.* Finally, what can be said about the problem which was set in the first step? Has the problem been solved? Has the analogy proven successful? If no solution has been found, what are the resulting consequences for the source and the structure of the analogy? Holyoak and Thagard list the only three possible verdicts: 'the source can be applied to the target, it should not be applied to the target, or it can be applied to the target with modifications' (1995: 133). In most cases, the third verdict will apply. They further suggest that analogies cannot only solve the particular target problem, but also lead to deeper and more general principles of explanation (so-called schema induction):

The message, then, is simple. Analogy is not a surefire shortcut to expertise, far less a substitute for careful thinking and detailed study of a new domain. But when it is used carefully—when a plausible analog is selected and mapped, when infer-

ences are critically evaluated and adapted as needed, and when the deep structure of the analogy is extracted to form a schema—analogy can be a powerful mental tool. (Holyoak and Thagard 1995: 137)

A reading grid

In the following chapters, the above algorithm will be used as a reading grid for studying historical occurrences of analogical reasoning. However, it is important to keep in mind that this algorithm does not necessarily provide the description of a chronological sequence, but rather a rational reconstruction of what happens when an analogy is made. For example, a primatologist studying chimps will most likely start from his source (step 2) and then see whether it can shed any light on a palaeoanthropological problem (step 1). On top of that, not all steps will be as neatly discernible; source retrieval often coincides with mapping, evaluation and conclusion are not always separable. Some steps are thus skipped while others merge. Clearly, this algorithm only serves as an ideal-typical sequence. Yet despite the discrepancy between actual chronology and logical ideal, the heuristic value of this algorithm remains untouched as it renders explicit the practice of analogical reasoning. The questions associated with each of these steps are the ones all historical texts will be subjected to in the following chapter. The analogical algorithm is a question sheet to be used during the historiographical interrogation of archaeology and primatology in order to reconstruct processes of reasoning.

Three objections can be made against this algorithmic reading grid. First, it can be questioned whether logic is a good access to understanding scientific practice. The anarchistic philosopher of science Paul Feyerabend, always wary of methodological rigidity, formulated it as follows:

The ideas which scientists use to present the known and advance into the unknown are only rarely in agreement with the strict injunctions of logic or pure mathematics and the attempts to make them conform would rob science of the elasticity without which progress cannot be achieved. (Feyerabend 1975: 303)

This is a valid point: creative thought in science often goes beyond the well-trodden paths of logic. However, this critique only applies when logic is seen as normative framework, not as a descriptive device. It might not always be possible, nor desirable to force ongoing reasoning into the straitjacket of logic; yet it is certainly possible to use some of that logic as a flexible instrument for describing arguments. My use of logic comes down to a heightened awareness of cognitive processes, not as an orthodoxy to be imposed upon science. I do not want to rob science of its elasticity but make this very elasticity visible.

A second criticism is somewhat more tenacious. How can such modern algorithm avoid the old problem of anachronism? In order to lay bare the skeleton of analogical reasoning in archaeology and primatology, I substantially rely on ideas and principles from inductive logic, philosophy of science and cognitive psychology. If we now want to study historical occurrences of analogy from the first half of the nineteenth century onwards, how can we confidently use an analytical

framework which was only formulated during the last fifty years without forcing our historical texts in a contemporary straitjacket? The ambition to use modern logic for studying old texts looks suspiciously like the historian's worst anathema—that of anachronism, whereby past events are studied and perhaps even judged in terms of later insights. There are a number of rebuttals to this objection. Firstly, although the logical literature I have been drawing upon is relatively recent, the standard classical treatment of analogy goes as far back as Aristotle (Lloyd 1966) and the general principles of inductive logic were laid down by J.S. Mill, a contemporary of the Victorian social evolutionists (Jevons 1870). So, in strict chronological terms, the historical manifestations of archaeological analogy do not precede the logical inspiration of our treatment. Secondly and more importantly, the allegation of anachronism would be justified if the present aim was to write a traditional history of science which places scientific ideas in their historical 'contexts'. Relying on recent logic would in this view seriously distort a genuine understanding of the past. Yet, as said in the introductory chapter, rather than aiming at a *contextual* historiography, my approach is in first instance a *conceptual* one which tries to disentangle some of our inferential structures. This is certainly less ambitious than the contextual, historicist programme because the underlying question is not 'why did some ideas appear in this or that context?' but simply 'what were those ideas precisely like?'. A thorough understanding of ideas seems in any case crucial for any further interpretation. Rather than blurring the historical uniqueness of past arguments, an analysis from inductive logic renders it more visible. Thirdly, the use of logic would be unduly presentist if the main aim was to *criticize* certain forms of analogy. Although criticism will not be entirely absent from this work, it first of all attempts to *elucidate* historical manifestations of archaeological analogy. Just as an Egyptologist will need to translate his hieroglyphs into a more familiar language, the historian of analogy must make a translation of past arguments into a comprehensive idiom. Inductive logic, then, serves as an interpretative grid for understanding such analogies.

A third objection could be: since analogy has been largely discussed in ethnoarchaeology, my treatment of Victorian anthropology or postwar primatology will inevitably show an ethnoarchaeological bias. There is a certain degree of plausibility in that remark. My interest in the logic of analogy was indeed sparked by my readings in ethnoarchaeology, especially by what Alison Wylie had written, and at the outset I was more familiar with the analogy debate in archaeology than elsewhere—one cannot deny one's own disciplinary roots. However, other, more neutral strands of thought have equally coloured my vision. My inspiration in cognitive psychology, for instance, was unrelated to any of the disciplines I wanted to study. Ethnoarchaeologists have drawn upon the logic of analogy—so have I, but with different purposes. It is not because one discipline has taken the effort to delve into the analogy debate in philosophy, that it should be forbidden to do so for a historian of science. Even if ethnoarchaeology had not made this logical excursion, I would have been referring to it. So if my attitude towards analogy sounds somewhat ethnoarchaeological, it is because of this shared reliance on inductive logic, less because of my disciplinary bias.

These potential objections warn against the danger of forcing texts into a strait-jacket, no matter whether it is logical, anachronistic or disciplinary. The analogical algorithm, I believe, overcomes these criticisms. If used cautiously and flexibly, it allows to study a great variety of texts without doing injustice to their individual complexity and historical identity. Indeed, the question sheet I devised only tries to articulate historical arguments. In this sense, my reliance on inductive logic, philosophy of science and cognitive psychology is closer to hermeneutics than to a narrow logical positivism. Gadamer, whose hermeneutic philosophy did not provide a shortcut to better understanding but simply a clarification of the process of understanding, said: ‘Every statement has to be seen as a response to a question and [...] the only way to understand a statement is to get hold of the question to which the statement is an answer’ (Gadamer 1991: 332). Or briefly, ‘all statements are answers’ (333).

Since most applications of logic in archaeology emerged during the strongest positivist moments of processual thought (e.g. Watson, LeBlanc and Redman 1971), it may come as a surprise to find logic here described as a hermeneutic instrument. Yet logic has many faces. Whereas processual interests zoomed in on deductive logic, my conceptual historiography relies on inductive logic so that from a way to reach absolute truth, the present use of logic is restricted to that of a historical device. In a hermeneutic way, inductive logic helps to understand historical texts, that is, it helps to find the questions to which every statement is an answer. Hermeneutic philosophers have stressed that these questions don’t need to be the original ones the historical authors had in mind, but simply the ones the interpreter formulates in order to make a text understandable and comparable. The questions defined in the analogical algorithm above serve that purpose.

Inductive logic has a further hermeneutic quality. Just as hermeneutics comes down to the reflexive, theoretically-heightened awareness of what spontaneously happens when we interpret, inductive logic makes explicit what occurs tacitly in day-to-day reasoning. Our lives are filled with all sorts of inductive arguments and analogy might be the most common one. An analysis from inductive logic, such as in the rather gratuitous examples I have used in this chapter, always articulates an intuitive knowledge. The main value of inductive logic resides in its capacity to lay bare the underlying but pervasive reasonings such as they occur in historical texts. Yet which are these texts?

A corpus of texts

For the analysis of analogies in the history of prehistory, I have selected *more or less elaborate arguments from the Anglo-American world which consciously use contemporary source contexts to make constructive inferences about prehistoric behaviour*. This has to be further specified. With *more or less elaborate* I suggest that the argument should be more than a passing reference to ethnography or primatology but a substantial text, consisting of at least a paragraph, but preferably an article, a chapter or a book. However, these publications are not evenly spread. My treatment of nineteenth-century analogies will be heavily dominated by books, the one of twentieth-century analogies by articles and chapters. Besides the easier availability

of nineteenth-century books than articles, this imbalance is also due to a more substantial factor: the perceived nature of scientific knowledge and the appropriate publication for scholarly discussion. It would lead us too far to discuss this in any depth, but it seems as if nineteenth-century science, despite its acrimonious debates, was more given to an objectivist notion of truth whereby knowledge was stable and progressed by accumulation. Monographs, often multi-volume tomes, were the building bricks of the scientific edifice. The genre of the scientific article became more important in the first half of the twentieth century (although it found its origins in the earlier proceedings of learned societies), and knowledge was increasingly being experienced as unstable, negotiated, and open to revision. Fixity thus became fluidity. If I discuss more books for the nineteenth century, it is because they were proportionally more numerous and more important in their time, and more easily accessed in mine.

With *Anglo-American*, I restrict myself to analogical arguments which have been formulated in Britain and North-America, or which at least have been influential there. Debates from other national traditions will only be treated to the extent that they had a demonstrable impact on the Anglo-American world. For instance, it is impossible to speak about early nineteenth-century archaeology in Scotland without mentioning the role of Scandinavian scholars like Thomsen and Worsaae; or to deal with late nineteenth-century Palaeolithic archaeology in Britain without considering the chronological framework set out by the Frenchman De Mortillet. I realize that this decision misses out a number of potentially very interesting research traditions, but feel confident in doing so given the relative autonomy, now and then, of the debates discussed (Van Reybrouck 1994a; 1994b). Victorian sociocultural evolutionism was, with the exception of Morgan, really very British, and even London-based, closely aligned to the Ethnological Society and the notorious X Club. In Berlin and Paris, anthropology's two other capitals at the time, the tenor of debate was very different, although the life-histories of individuals like Westermarck and Boas linked it with the English-speaking world. The same holds true for ethnoarchaeology which was for nearly fifteen years strictly American; only in the early 1980s did it arouse a certain but very different interest in Britain, and later also in countries like France, Germany, Belgium, and the Netherlands. Primate modelling, too, was largely a North-American endeavour; other ethological traditions like the British and the Dutch attitudes were generally more sceptical. Granted this relative isolation of each debate as national traditions, my restriction to the Anglo-American world, therefore, was more than an arbitrary decision.

The criterion for *conscious use of source contexts* demands that the argument explicitly refers to observations from the present world in order to draw inferences about the past, implying a considerable empirical content. Whether the argument is called 'model', 'analogy' or whatever else has no importance. Strictly theoretical studies of analogy will only be treated secondary. Texts have also to meet the following criterions to be qualified. Their use should be *constructive*, i.e. analogies are believed to contribute positively to our understanding of the past, in contrast with more restrictive uses of analogy as cautionary tales (although such criticism

can be indirectly constructive). The argument should say something about *behaviour*, in the widest sense of the word: social, technical, ecological, cultural, ideological. This implies that analogies which are only invoked to explain aspects of anatomy, such as commonly done in comparative zoology, will not be considered. (The distinction will sometimes appear artificial; studies of early hominid locomotion, for example, verge both on behavioural and anatomical aspects; studies of primate dentition implied a behavioural component.) Clearly, an emphasis on behaviour goes to the heart of the inductive challenge because, unlike the gratuitous example of the polished flint axe, here the notions of uniformitarianism and unambiguity are much more problematic as they relate to human uniqueness versus the possibility to extrapolate across cultures or even species. It is one thing to compare tools, but quite another thing to compare societies.

Finally, the argument makes a statement about behaviour in *prehistory*. I use the term prehistory to indicate both the field of Lower and Middle Palaeolithic archaeology where hunter-gatherer ethnoarchaeology has played an important role, as well as the palaeoanthropology of the Plio-Pleistocene where primate modelling made its largest contribution. The emphasis is thus on the oldest periods of human prehistory; where necessary, however, my discussion will also include more recent periods. It is, for instance, hard to talk about the role of analogy in functionalist archaeology of the 1940 and 50s without talking about Mesolithic and Neolithic studies, just as one cannot turn a blind eye to the role of ethnoarchaeology in contextual archaeology because it focused on more recent prehistory. My loose definition of prehistory is thus dictated by the choice of studying a method rather than a period.

On the basis of these criteria, a corpus of texts was assembled that relates to the three large debates of analogy use in the study of prehistory: postwar primate modelling, the Victorian comparative method and ethnoarchaeology. The importance of the second half of the nineteenth century cannot be easily dismissed, not simply because it marked the birth date of both cultural anthropology and prehistoric archaeology, but also because that birth was one of Siamese twins: both disciplines constituted each other and the comparative method was the backbone they shared. Though sociocultural evolutionism is generally delineated between the establishment of human antiquity around 1860 to Sollas' synthesis of 1911, I decided to take one step back to study the impact of the three-age system in the second quarter of the nineteenth century. After a period of relative silence between 1910 and 1960, ethnoarchaeology meant an enthusiastic return to analogical reasoning in archaeology, even if it was already initiated before the New Archaeology and has fallen from grace since. Simultaneously, the discussion on primate models emerged from an implicit palaeoanthropological agenda in the mid-1950s, over a long period of referential modelling, to a range of alternative approaches presented nowadays. Each of these debates will receive separate treatment in the subsequent chapters. Though they all knew their heyday and classical formulations, their less glorious moments of growth, doubt and criticism will be studied with equal attention, if only to avoid the danger of typifying a period by its most extreme episodes and canonical formulations.

The quality of the corpus of texts I decided to work with depended for a good deal on its size. In each of the cases, an attempt was made to study as much material as was practically possible. The debate on postwar primate models covers most of the relevant publications; the comparative method is represented by what were, and still are, all the classical texts; the discussion on analogy in the twentieth century and in particular ethnoarchaeology makes use of a large sample of the vast literature. When not all texts could be considered, care was taken to study the most relevant and influential ones. For instance, the 1980s saw the publication of hundreds of taphonomic and ethnoarchaeological studies in archaeology. But since most of them worked within the same logic set forward by Binford, the latter's work and an adequate sample sufficed. If the representation of each debate is thus somewhat skewed, it is also because the debates themselves are skewed: there are simply much more articles on ethnoarchaeology than on primate modelling. An even representation would have been required if I were to perform statistical analyses, but this was not the case. The interest was in arguments, not in their relative degrees of abundance.

A choice of focus

In practice I used the reading grid defined above as my guide through the corpus of texts. The questions associated with each step of the analogical algorithm were put on a fill-in sheet and for each publication this questionnaire was filled in with appropriate passages and quotes from the text, alongside my own personal remarks and cross-references. The visual diagram (figure 2 and 3) was often helpful when harder nuts needed to be cracked. When reasoning was complex, badly phrased or simply poor, this diagram was very useful to clarify the arguments.

Eventually, this reading grid stimulated nothing else but classical 'close reading' of a corpus of texts, albeit in a somehow more systematic way. Were it not so vague, close reading might be the appropriate name of what I did. Going through a text, catching its structure, asking questions of it, filling in the query list, dissecting its arguments, going again through it, drawing a logical diagram, checking its sources, comparing it with contemporary texts, comparing it with texts that preceded and followed it, leaving it for a while, going back to it several months later, reading it again, wondering whether I had been right, revising my opinion—this is what I understand by close reading, trying to get at what the author intended to say. If this sounds like a minimal definition of reading, one is surprised to see how often such minimum is easily bypassed. Reading a text is certainly, as hermeneutic thinkers say, the act of acquiring an understanding *par excellence*.

In my own writing about each of these periods, I have remained fairly close to the original texts. The pendant of close reading is heavily quoting. It is very easy to reduce entire debates to a single caricature (and this has often been done, especially in the case of the comparative method) based on a couple of extreme utterances. However, close reading through a debate's constituent texts invariably teaches that there is much more complexity, diversity, and *finesse* than originally assumed. Apart from the occasional despondency, the awareness of this richness generally leads to nuance. This can also bring a new danger with it, i.e. that one

starts to loose oneself in the minutiae of a text. Once you are familiar with a debate and go through a publication for the third or fourth time, it becomes possible to comment on every passage, sentence or even word in it. The author's sources of inspiration, the format of argumentation, the particular vocabulary start to become so well known that one could write footnotes to every line. Whereas this might be gratifying, it also risks that historical discussion of a publication simply turns into an annotated text edition.

In general, then, I have tried to find a balance between microscopic scrutiny and sweeping generalization. Whereas the individual sections often go into considerable detail, the conclusions to each of them and to the chapters in general tend to take a wider perspective. To use a photographic metaphor: I have mainly used the telephoto lens, while often changing to the wide-angle when appropriate. I hope that my focus was sufficiently variable according to the circumstances, that it was, so to speak, a powerful zoom lens rather than a set of static lenses. Eventually, however, the prior knowledge of the reader decides whether anything is treated with too much detail or too much dashing. A primatologist might wonder about the purpose of dissecting Tylor's last articles, an archaeologist might enjoy this discussion, whereas a historian of anthropology would perhaps like to see more circumstantial evidence. My treatment of texts avoids becoming unnecessarily technical or unceremoniously superficial, fully realizing, however, that writing about several disciplines and the way they developed over two centuries inevitably entails abstraction. What may have been lost in depth is hopefully regained in width. The panorama is vast, but close-ups are required to appreciate it.

Conclusion

Analogical arguments permeate all historical sciences including palaeontology and geology. When it comes to the reconstruction of early hominid behaviour, Palaeolithic archaeologists, ethnoarchaeologists, palaeoanthropologists and primatologists have developed analogies on the basis of the available ethnographic and primatological evidence. This chapter has first of all attempted to provide an idiom to talk about analogies. Analogies have been defined as inductive arguments which on the basis of given similarities between two entities predict further similarity between these entities. Being inductive, analogies are to be seen as more or less plausible instead of valid or false and analogical conclusions as probabilistic instead of true. Still, there are a number of criteria which help assert the strength of an analogy, especially when issues of relevance are considered. Relying on this logical discussion, an analogical algorithm consisting of five distinct inferential steps serves as an important heuristic and hermeneutic device in the analysis of analogies from three different time-slices in the history of archaeology: the Victorian comparative method, modern ethnoarchaeology and post-war primate modelling. It is to these that we now must turn.

The comparative method

The comparative method of the nineteenth-century evolutionists is without doubt the best known and best studied example of a debate on ethnographic analogy (Burrow 1966; Stocking 1987; Trigger 1989; 1998). During the past forty years the historiographical interest for it was diverse and intense. As part of the evolutionist renaissance in anthropology, the 1960s witnessed numerous re-editions of classical nineteenth-century texts and an avidity to study those who were revered as the founding fathers of modern anthropology (Carneiro 1967; Harris 1968; Andreski 1969; Nisbet 1969). When the grand narrative of evolutionism was subjected to a more distanced approach in the late 1980s, re-appraisal made place for critical historiography and even deconstruction which turned the founding fathers into a fascinating but ambivalent legacy (Ingold 1986; Stocking 1987; Trautmann 1987; Kuper 1988; Bowler 1989; McGrane 1989).

Why then write another study of the comparative method if authors like Burrow (1966) and especially Stocking (1987) have done this so eloquently? The reason is that my focus is different. Whereas the aforementioned authors remain on the polemical context of the debate, my approach also wants to detail the logical structure of the arguments and the debates. There has, of course, been an interest in the logic of the comparative method (particularly Nisbet 1969), but this tends to treat all variety together under the same, unified banner of ‘The Comparative Method’. The approach followed here navigates between a strictly historical and a strictly logical treatment. Building on previous scholarship, it pays a lot of attention to the varying polemical contexts, but it also tries to treat the various forms of reasoning as analogical algorithms.

Understanding the varying polemical contexts is important since all too often the comparative method is treated as a package which arrived somewhere in the early 1860s and did not change until the demise of evolutionism at the turn of the century. The comparative method, however, had a history of its own—both before and after 1860. It emerged from an Enlightenment legacy and further developed during the evolutionist decades of the second half of the nineteenth century. Stocking, for instance, has drawn attention to the link with mid-century Prichardian ethnology. Another intellectual tradition which influenced the sociocultural evolutionists and which I would like to stress is the Scandinavian preoccupation with the chronology of artefact assemblages which resulted in the famous three-age system. It was because the first of the three ages, the Stone Age, was believed to be a universal phenomenon that ethnographic parallels with contemporary Stone Age societies could be drawn—an idea put in practice by authors like Nilsson and Wilson and which influenced later evolutionists.

For the period after 1860, the role of the degenerationist critique has been seriously underestimated in the history of the comparative method. A study of evolutionism therefore also requires a more balanced appraisal of its competing tradition, i.e. degenerationism.

The next step consists of moving from a contextual to a conceptual analysis. In particular, I want to show how the criterion of similarity was highly valued, and how it even turned into the single-most important yardstick for appraising analogies as the century moved on. The various debates on the three-age system, the antiquity of man, and the degenerationist critique contributed to the widespread belief among evolutionists that resemblances between prehistoric and exotic savages were taken-for-granted. Parallel to that development, the inferences drawn evolved from piecemeal to wholesale. For Thomsen and Nilsson, ethnographic analogy first served to explain strange-looking tools; for Morgan and Sollas, it served to explain entire stages of human civilization. Nineteenth-century anthropology thus shows how the increased conviction of resemblance resulted in ever more daring projections. This logico-historical development, I believe, has been hitherto understudied in the scholarship on sociocultural evolutionism.

A final reason for this chapter is that it provides the necessary background to speak about the use of analogy in the twentieth century. Primate modelling and certainly ethnoarchaeology cannot be properly understood without prior knowledge about the Victorian debate. Ethnoarchaeology developed explicitly in reaction to the comparative method, primate modelling was implicitly indebted to it. A re-analysis of this nineteenth-century debate is therefore needed. And to do so requires a brief look at the pre-nineteenth century interest in non-European primitives.

Early ethnographic parallels

A faint awareness of resemblances between the present-day manners of neighbouring societies and the assumed history of one's own society has been an integral part of Western thought since Antiquity (Burrow 1966: 11; Nisbet 1969: 192-4).¹ Aristotle and Thucydides found in contemporary barbaric peoples remnants of what the Greeks once had been, and Tacitus thought the Germans represented the Romans of long ago. Yet such reasoning received some crucial impulses during the second half of the fifteenth century. The fall of Constantinople in 1453 and the discovery of the New World in 1492 provided Western Europe with two critical resources to speculate about human ancestry: Byzantine scholars fled to the West and imported a great number of classical texts, thus contributing to the Renaissance interest in Antiquity; explorers shipped at least a

1 For this section I have particularly relied upon Piggott's work on the antiquarian tradition (1956; 1976; and especially 1989), Daniel (1967) and Lynch and Lynch (1968). Orme (1973; 1981), Klindt-Jensen (1981), and Hodder (1982a) provide some additional information on the history of ethnographic analogy, whereas Grayson's (1983) treatment of the older phases of the establishment of human antiquity is still unsurpassed by more recent scholarship (like Van Riper 1993). Burrow (1966), Lemaire (1986) and Stocking (1987) give succinct but useful background on the Scottish Enlightenment, Laming-Emperaire (1964) still stands out as a classic study on the origins of prehistoric archaeology in France, only to be recently sided by Schnapp's impressive monograph (1993).

thousand Indians to Europe in the decades after Columbus (Piggott 1989: 73). In a sense, from the East came books and from the West savages—both proved to be the key components out of which antiquarian speculation on the origin of human nature were crafted (Lemaire 1986: 20-2).²

Sixteenth-century artists were among the first to draw implications out of this new context. Ancient Britons and American ‘savages’ were kindred iconographic themes and the depiction of Britain’s ancestors as primitive natives was a favourite theme in the last quarter of the sixteenth century. In 1575, the Flemish artist Lucas de Heere, temporarily resident in London, produced a fine watercolour depicting two ancient Britons. Partly inspired by classical texts, the painter had also shown a keen interest in overseas natives such as Eskimos (Piggott 1989: 75, plate 16; Schnapp 1993: 150). In 1586, the painter John White took part in the Virginian expedition where he made accurate watercolours of the aboriginal inhabitants. White eventually became Governor of Virginia and in 1590 his illustrations were engraved by the printer Johan-Theodoro de Bry, a native of Liège living in Frankfurt-am-Main, who published them together with Thomas Harriot’s *Briefe and true report of the new found land of Virginia* as the first part of his *America* (which also incorporated engravings based on Jacques le Moyne’s drawings of Florida natives). The encounter between Englishmen and Indians in Virginia redefined the notions of savagery and civility (Sheehan 1980); ethnographic sketchbooks became as important as classical texts to depict the ancient Briton.

The impact of these illustrations was considerable (Piggott 1989: 85-6; Moser 1998). In the seventeenth century De Bry’s engravings were widely reproduced and distinguished intellectuals drew upon the parallel between primitive and primeval life. Robert Burton, the author of *The Anatomy of Melancholy* from 1621, recommended the lecture of reports on American natives as a cure against gloominess. He held that the Germans described by Caesar and Tacitus were as uncivilized as the natives of Virginia. Thomas Hobbes contended in 1651 that the brutish life of primitive man could still be seen at work at the other side of the Atlantic. And from Locke came in 1690 the famous dictum: ‘In the beginning all the world was *America*.’ Images of North-American, especially Virginian, savagery were thus incorporated in philosophical discourses on man’s primeval condition throughout the seventeenth century.

Classical authors remained important and provided the most immediate resource for reconstructing the time period prior to the Roman Conquest. For instance, the popular idea that the ancient Britons painted their bodies was one of those textual inferences. Yet reliance on the written word did not preclude an interest in the ethnographic present. Antiquaries were at pains to prove that the skin-covered boats mentioned by Caesar and Pliny were essentially the same as the *currachs* still seen in Ireland (Piggott 1989: 63-4). At the time, ‘Ireland and America involved, for the English, similar experiences’ of technological and cultural inferiority (Sheehan 1980: 55). The antiquarian reconstruction of the ancient Briton was a composite picture drawing upon ethnographical images from

2 Perhaps this aphorism meets more the Platonic virtue of beauty than truth: books from Antiquity were also handed down by Moorish scholarship, by medieval libraries, etcetera. The fall of Constantinople was but one of the impulses for the Renaissance, though an important one at that.

the Americas, detailed descriptions of material culture given by classical writers, and corroboration of these by survivals within the British Isles.

By the end of the seventeenth century, knowledge on contemporary primitive life became influential in the solution of a long-standing problem: the recognition of thunderbolts as stone tools. In 1656 Sir William Dugdale interpreted the so-called ceraunia found in Warwickshire as weapons used by the ancient Britons before they had metallurgical knowledge. In 1686 his son-in-law Dr Plot suggested that prehistoric stone tools were hafted like the Indian examples. As the first Keeper of the Ashmolean Museum in Oxford, Plot had ready access to prehistoric and exotic examples of lithic implements. In a letter from 1699 (published in *Philosophical Transactions of the Royal Society* of 1713; cf. Daniel 1967: 39), the Welsh antiquary Edward Lhwyd who was Plot's assistant and successor at the Ashmolean, confidently juxtaposed British and recent North-American stone tools to show they had been used as arrowheads.

This antiquarian scholarship in Britain fell soon into oblivion. Piggott (1989: 31-2) has persuasively argued how after the publication of Newton's *Principia Mathematica* in 1687, the non-mathematical disciplines lost social prestige and scientific credibility while more attention was given to abstract generalizations (cf. Piggott 1956; 1976; Lynch and Lynch 1968). Plot had once been the secretary of the Royal Society (in 1682), but after Newton was elected president in 1703, the antiquarian interests were paled. At the same time in Paris the *Académie royale des Sciences* drew more attention to the matter. The appearance of Michel Mercati's *Metallothesca Vaticana* in 1717, a work already written in the late sixteenth century which indicated thunderbolts as primitive implements, proved influential. Few years later, in 1723, Antoine de Jussieu presented a paper to the *Académie* in which he compared prehistoric stone tools with some recent examples from America and Canada to infer that '*les pierres de foudre*' had been man-made instruments.

Yet ethnography could do more than clarify stone tools. In the first half of the eighteenth century it became clear that it held broader relevance. Joseph-François Lafitau, a French Jesuit who had spent five years with the Indians in Canada, published in 1724 *Mœurs des sauvages américains: comparées aux mœurs des premiers temps*, a telling title. Ethnographic evidence was according to Lafitau a means to find the primitive traits of humanity, particularly in relation to religion. The epitome of Jesuit scholarship, he constantly moved between his knowledge of classical literature and his own ethnographic observations. On most occasions the Ancients elucidated the savages, sometimes the savages elucidated the Ancients, but the aim was to find the characteristics of primitive religion. Lafitau's work is interesting as it marked a new tendency to incorporate information on savagery in theologically-philosophical speculation on human development.

Lafitau was not alone. Montesquieu wrote *De l'esprit des lois* in 1748. Goguet published his often reprinted *De l'origine des lois, des arts et des sciences, et de leurs progrès chez les anciens peuples* in 1758.³ The Danish scholar Jens Kraft wrote a work in 1760

³ An English translation appeared three years later: *The origin of laws, arts, and sciences, and their progress among the most ancient nations* (1761).

which was translated in German under the title: *Die Sitte der Wilden, zur Aufklärung des Ursprungs und Aufnahme der Menschheit*. In all these works, comprehensive histories of mankind were furnished with evidence from contemporary savages. Though Lafitau was exceptional in having observed North-American Indians himself, Goguet deserves credit for his clear defence of comparative ethnography (1758: xxxii-xxxiii):

J'ai crû que la conduite de ces Nations [sauvages] pouvoit nous fournir des lumières très-sûres & très-justes sur l'état dans lequel se seront trouvées les premières peuplades immédiatement après la confusion des langues & la dispersion des familles. [...] On doit juger de l'état où a été l'Ancien Monde quelques temps après le déluge, par celui qui subsistoit encore dans la plus grande partie du Nouveau Monde, lorsqu'on en a fait la découverte.

Biblical notions like the Flood, the confusion of tongues and the dispersion of families were still authoritative, but the confrontation with ethnographic evidence eroded the Scripture's monopoly (Lemaire 1986: 191-5; McGrane 1989: 61-4). Goguet's use of the comparative method was part of a much broader history of civilization largely built on biblical evidence. Like Lafitau, he sought to reconcile '*les Relations des Voyageurs modernes*' with the '*Ecrivains de l'antiquité*' (xxxiv).

The tradition of composing universal histories of mankind with overseas evidence reached a tentative apex in the French and Scottish Enlightenment of the second half of the eighteenth century (Bryson 1945; Meek 1976; Lemaire 1986: 198-206; Stocking 1987: 10-9).⁴ Edinburgh-based thinkers like Robertson, Ferguson, Kames, Smith and Millar shared a great deal of their basic assumptions and ambitions with the Parisian *Lumières* like Turgot, Rousseau, Condorcet and Helvétius: an interest in the history of civilization; the conviction that this history was by and large progressive; the belief, biblical in origin, in the psychic unity of mankind; the observation of differential progress among the nations of the earth; and the resultant principle that what was seen in present-day primitive races was essentially similar to what had characterized the prehistoric races in Europe. Despite obvious physical differences, humans were believed to belong to a unique species endowed with mental faculties, which were, however, not everywhere actualized to the same degree (Stocking 1987: 19, 51). Turgot and Smith independently came up with a 'four stages theory' (Meek 1976) which dissected the history of civilization into four consecutive stages of socio-economic growth: hunting, pastoralism, agriculture and commerce. The model proved influential throughout the second half of the eighteenth century, particularly in Scotland (Lemaire 1986: 198; Stocking 1987: 16). Though the interest was more in 'the progress of civilization in Europe' than in 'the origin of civilization in savagery' (Stocking 1987: 19), ethnographic comparison was one of the pillars on which the edifice of the *philosophes* rested.

In sum, the idea of a comparative method emerged in several distinct contexts with quite distinct purposes; it entailed the image of ancient Britons by late-sixteenth century, mostly London-based artists; the identification of flint artefacts

⁴ These histories are often referred to as 'conjectural', which is a rather unfortunate term considering the empirical, factual programme at stake (Bryson 1945: 52; Meek 1976: 239). Goguet, for instance, stated: '*On a trop donné à la conjecture*' (1758: v-vi).

as prehistoric tools by curators of the Ashmolean museum and by French *académiciens* around 1700; and the reconstruction of *une histoire raisonnée de l'homme* by French and Scottish Enlightenment philosophers. In essence, the image of primitive and primeval savageness was based on the American Indian, but during the eighteenth century the exploration of the South Seas by Cook and the slow penetration of Africa provided new reports on non-western others (George 1958).

The use of ethnographic analogy continuously oscillated between two extremes: either a global image or a local inference. Artists like De Heere, White, and De Bry wanted to portray a broad visualization of the ancient Briton; antiquarians like Plot, Lhwyd and Jussieu worked on the much more circumscribed thunderbolt-problem; the *philosophes* were interested in the more general stage which a savage society embodied. From Renaissance to Enlightenment, analogies stood between illustration and interpretation, between projection and problem-solving, between image and argument—a tension which had important repercussions in the centuries to come.

How this heterogeneous tradition of ethnographic parallels in artistic, antiquarian, theological and philosophical contexts, with its amalgam of ideas on progress, psychic unity and comparison, its scriptural and classical underpinnings and its illustrative, interpretative and reconstructive motivations was translated into a consistent research programme at the birth of archaeology in the early nineteenth-century shall now be investigated.

The impact of the three-age system

The birth of scientific archaeology, traditionally associated with the Danish three-age system, is seen as a great leap forward compared to the conjectural schemes of the eighteenth century. This section aims to stress the basic continuity between Enlightenment philosophy and Scandinavian typochronology. It acknowledges fundamental innovations in the explanation of the Bronze and Iron Age, but shows how this has resulted in a dualist vision of prehistory. The three-age system resulted in a two-stage account of human civilization where a natural, universal, global Stone Age was in certain places ended by a series of local migrations during the metal ages. The impact of this dualism on the use of ethnographic analogy was profound, as the work of Sven Nilsson and Daniel Wilson shows. It was limited to the Stone Age and limited to technological comparison.

A revolution in antiquarian thought?

Despite the French and Scottish schemes, it was in Scandinavia that such sequential thinking was first applied to an elaborate archaeological data set. The story has often been told of how in 1816 C.J. Thomsen rearranged the rich materials of the National Museum of Danish Antiquities in Copenhagen by dividing them into a Stone, Bronze and Iron Age, a method explained by him in the famous *Ledetraad*

(1836) and corroborated by the stratigraphic excavations in Danish peat bogs undertaken by his successor J.A.A. Worsaae (Daniel 1943; 1967: 90-109; 1975: 38-55; Trigger 1989: 73-9).

The pioneering role historians of archaeology generally attribute to Thomsen has tended to obscure the long-standing tradition on which he himself built.⁵ Italian Renaissance scholars like Agricola, Aldrovandi and Mercati had already on the basis of their readings in Homer, Hesiod and especially Lucretius come up with the idea that there had been a time in human history where stone was used before metal—an idea they applied to the thunderbolts. Mercati even went so far as to suggest the existence of successive eras of stone, bronze and iron tools.

The Italic atmosphere which pervaded seventeenth-century France and the edition of Mercati's work in the early eighteenth century ensured a continuity between Renaissance scholarship and French Jesuit and Enlightenment science. Next to Jussieu's recognition of stone tools, Mahudel read a paper to the *Académie des Inscriptions* in 1730 where he accepted Mercati's three successive periods. In another paper read to that academy in 1734 the Jesuit Montfaucon, whose excavations of the megalithic tomb of Cocherel in Normandy had demonstrated the existence of a Stone Age, agreed wholeheartedly with this three-age system. And so did Goguet, the author of *L'origine des lois, des arts et des sciences* (1758) who argued that humans had first been using tools made from stone, later from soft metals like gold, silver and copper, and finally from iron (Grayson 1983: 13). By the middle of the eighteenth century in France, many scholars were prepared to accept a prehistoric era in which stone was used as the prime resource before the advent of metal and several of them believed in a sequence of Stone, Bronze and Iron Ages.

France provided the cultural ideal on which the Danish court and aristocracy modelled itself and late-eighteenth and early-nineteenth century Scandinavian scholarship was marked by a considerable French scientific import (Clarke 1968; Trigger 1989: 74). Kraft's *Die Sitte der Wilden*, for instance, was heavily inspired by Lafitau's *Mœurs des sauvages américains*. The extent to which Thomsen was acquainted with these French scholars is not clear but it may have been considerable. In a letter to a German scholar in 1825, he spoke of 'the validity of the *old* notion of first stone, then copper, and finally iron' (in Klindt-Jensen 1975: 52, emphasis added). Thomsen's three-age system came down to the systematic implementation of this old developmental scheme to an important but unordered collection of ancient relics. Gräslund (1987: 13-6) has indicated how Denmark and Sweden were the first countries to have substantial national collections of antiquities which were centralized in one place, instead of dispersed over provincial

5 The traditional defendant of the Scandinavian originality has been Glyn Daniel (1943; 1967). David Clarke, however, in the excellent but somewhat forgotten historical introduction to his *Analytical Archaeology* (1968: 4-11), has challenged this view by indicating the importance of Thomsen's conceptual forebears, a view which has increasingly won supporters (Gräslund 1981; 1987: 17-9; Grayson 1983: 11-14; Trigger 1989: 75 and particularly Rodden 1981). Though Daniel accepted the existence of some pre-nineteenth century predecessors, in later publications (1975; 1976) he still praises the northern antiquaries as the founders of prehistoric archaeology and Thomsen as the one 'who produced the revolution in antiquarian thought' (1976: 41).

Wunderkammer. Rather than deriving from ‘the genius of this one person’ (Daniel 1976: 41), it was more of a ‘historical accident’ (Clarke 1968: 10). The success of this ‘bundle of ideas’ laid ‘in its acceptability—it was the right thing at the right time’ (Rodden 1981: 65).

Yet there were differences. The work of Thomsen and Worsaae had far less universalist pretensions than that of the eighteenth-century philosophers. Inspired by the Romantic quest for a national past, Danish antiquarian interest was largely fed by a patriotic motivation, especially since international politics had challenged the very concept of Danish identity (Klindt-Jensen 1975: 58–61; Kristiansen 1981; Trigger 1989: 74). Providing an intelligible framework for the Northern past was the prime ambition, not the fabrication of a narrative on the origin and development of human civilization. Thomsen did never argue that all regions of the world had gone through a similar succession of Stone, Bronze and Iron Ages. The three-age system simply served as a local classification for Scandinavian prehistory, and it was only at a later stage and through the work of other scholars that it became a global sequence.

Parallel to this preference of the local over the global, there was a preference of migration over evolution. Whereas eighteenth-century speculations explained the different stages of humanity’s development as a natural succession, Thomsen and Worsaae regarded the Bronze and Iron Age as the result of accidental invasions (Daniel 1975: 45; Stocking 1987: 72). The universal stratum of the Stone Age had ended abruptly with the invasion of a new race with much higher technological standards; and this race was eventually to be replaced by another people who possessed the knowledge of iron metallurgy. Terms from Thomsen’s seminal paper (1836; translated 1848) such as ‘imitation’, ‘international connexions’, ‘traffic’, ‘emigration to the North’ make clear that his concern was with regional diffusion and migration rather than universal evolution.

In sum, the sequence stone-bronze-iron was old, but the explanation of it was new. For Thomsen the three *ages* did not represent three *stages* of successive growth. Nonetheless, an implicit distinction was drawn between the Stone Age on the one hand and the Bronze and Iron Age on the other. Since polished stone axes from Denmark were comparable with specimens from across Europe and since they could still be found among present-day overseas savages, the Stone Age was believed to represent a universal primeval stage. But golden *lunulae*, bronze *luræ* and iron rapiers were finds with very few counterparts beyond the Danish peat bogs and were thus attributed to local processes. The dualism between the Stone and the metal ages was one of universal nature versus local culture, of slow process versus rapid change, and of evolution versus migration and diffusion. The three-age system thus came down to a two-stage view of human development.

Clearly, such dualism heavily influenced the use of ethnographic analogy: for the Stone Age it could be useful, for the metal ages it was to no avail. Thomsen noted that the earliest inhabitants of Denmark which had belonged to the Age of Stone ‘must have borne a resemblance to savages’ (1848: 64). Comparisons could be drawn for the Stone Age as this was a universal phenomenon. In letter to a Polish colleague, he wrote that in the earliest period great parts of Europe were ‘in-

habited by actually very similar and very primitive races' which 'correspond[ed] to the wild North Americans in many respects' (in Rodden 1981: 58). Worsaae pointed to the peoples of the South Seas to illustrate that stone tools could be used as efficient implements (Daniel 1967: 99). He also used contemporary descriptions of Brazilian Indians felling a tree as indications for prehistoric wood-cutting (Klindt-Jensen 1981). This taken-for-granted parallel between Stone Age people in the past and in the present was also reflected in both Thomsen's and Worsaae's museum layouts. Thomsen placed Danish flint axes side by side to South Pacific hafted stone axes. Worsaae, too, after his appointment as museum director in 1865, arranged the ethnographic artefacts so as to be of value as comparative material for the Scandinavian prehistoric artefacts (Klindt-Jensen 1976). But compared to Lafitau, Goguet and Kraft who had each built an entire *histoire universelle de l'homme* on the basis of ethnographic parallels, the Scandinavian antiquaries were far more parsimonious in the use of the comparative method (Ravn 1993; Klindt-Jensen 1976; 1981). It only served to explain the function, efficiency, and hafting of stone tools.

Despite its local emphasis, the three-age system started to be applied across Europe in the subsequent decades. Bruzelius introduced it to Sweden, Lisch to Germany, Wilson to Scotland and Keller to Switzerland. It became the unitary framework for understanding similarities and differences in European prehistory. To understand its role in the Anglo-Saxon world and the impact it made on the use of ethnographic parallels, we have to study the work of Sven Nilsson and Daniel Wilson.

The dualism of Sven Nilsson and Daniel Wilson

Few authors are at first sight so divergent as Sven Nilsson and Daniel Wilson. The one was a Swedish biologist, deeply rooted in the Enlightenment study of natural history, the other a Scottish antiquarian involved with the Romanticist study of the national past. And yet a comparison of their intellectual works is promising, as both enriched the Danish three-age system with ethnographic reasoning. On top of that, both were read by the younger generation of social evolutionists and influenced them to a considerable extent. Nilsson was translated into English by no one less than John Lubbock himself. Wilson was at least studied by the evolutionists, and his impact may have been larger than traditionally assumed (Kehoe 1998).⁶ In a sense, it is fair to say that the work of Nilsson and Wilson functioned as a bridge, both in time and by theme, between the Danish antiquarians and the British evolutionists. In time, since their work spans the second quarter of the nineteenth century. In theme, since it moved from artifact classification to cultural reconstruction.

6 Wilson's contribution to the emergence of prehistoric archaeology is still a matter of debate. Generally forgotten in brief historical reviews, Alice Kehoe (1991; 1998) has recently brought him out from behind Lubbock's shadow who is generally accredited the role of inventor of the discipline. Her argument is that Wilson did not have the social and academic credentials to belong to the London circles where Lubbock's star shone, that he, however, played a foundational role, that he coined the term prehistory, but that he was forced into overseas exile since no academic posts were available to him in Britain. In her view, Lubbock would even have plagiarized some of his work (but see Trigger 1994 for a sharp reply).

Sven Nilsson is one of the most intriguing and at the same time most poorly understood personalities in the history of archaeology, perhaps simply because he was not an archaeologist in the strict sense like Thomsen and Worsaae were.⁷ As a zoologist who had studied under Cuvier, he became professor of zoology and director of the zoological museum in Lund and dominated Swedish biology during the first half of the century. Ever since Linnaeus, Sweden had become an important country for natural history and taxonomy and Nilsson had won his spurs with several volumes on Scandinavian fauna, particularly birds, molluscs and fishes. As an introduction to a new edition of his standard work on the fauna of Scandinavia in 1834, he wrote an essay which dealt with the life of prehistoric hunters and fishers. He expanded these ideas and inserted a long treaty on the origin of the Nordic Bronze Age in his *Skandinaviska Nordens Ur-invånare*, published between 1838 and 1843. In 1863 the section on the Bronze Age was translated into German; in 1868 the section on the Stone Age appeared in English, as well as a French translation of the entire work. Lubbock's English translation appeared under the title *The Primitive Inhabitants of Scandinavia: An Essay on Comparative Ethnography* (Nilsson 1868).

Daniel Wilson, an antiquary whose early interests had been in the urban history of his hometown Edinburgh, published *Prehistoric Annals of Scotland* (1851) and, after his move to North-America, *Prehistoric Man* (1862), works which brought together all available facts on prehistory in Scotland and North-America. Written a century after Robertson's *History of Scotland* from 1751 and *History of America* from 1777, Wilson's books could be regarded as the archaeological pendants to these. Yet Wilson was no product of the Scottish Enlightenment. It is telling that on the rare occasions he refers to one of his compatriots, it is to the epitome of Scottish romanticism, Sir Walter Scott, rather than to any of the Enlightenment thinkers. Wilson belonged to this first generation of archaeologists who, though heirs to the Enlightenment, were more directly driven by the spirit of romanticism and patriotism which had also characterized Danish antiquaries (Kehoe 1998: 4-20). He was a devoted scholar who wanted to bring archaeology beyond the stage of a 'frivolous pastime', 'solemn trifling' and 'popular trammels' (1851: I, xxiv) in order to establish its value as 'the indispensable basis of all written history' (I, xx).

How did both authors relate to the three-age system? Nilsson was acquainted with the system and used concepts like Stone, Bronze and Iron Age, though he affirmed his relative independence from Thomsen (1868: xlvii). His classification of artefacts was first of all functional, not typochronological, assigning tools to categories such as 'implements for hunting and fishing', 'carpenter's or mechanic's tools' and 'ornaments'. Nilsson stressed that antiquities represented 'the fragments of a progressive series of civilization, and that the human race has always been, and still is, steadily advancing in civilization' (lvii). 'A progressive series of civilization', 'the human race', 'steadily advancing', how different does this sound after reading Thomsen! Nilsson was much more indebted to the eighteenth-century developmentalists than his Danish colleagues. He firmly believed in the unity of

⁷ The best work on Nilsson has regrettably remained untranslated from Swedish (Regnell 1983).

the human race and the superiority of civilization: ‘Nations spring into existence, and in their turn, decline and fall; but civilisation and humanity are steadily progressing’ (lix). Adopting the gradualist theory of socio-economic growth set forth by Turgot, Smith and Ferguson, he said that ‘every nation has had, or has, four stages to pass through’ (lxiv) and described the hunting and fishing, pastoral, agricultural and commercial stages of civilization (Kuper 1988: 64).

As can be expected, Wilson’s intellectual connection was first of all with Thomsen and Worsaae. In his first monograph, he said that the ‘Danish antiquaries have surpassed all others in the value and extent of their researches’ (1851: I, 10). Worsaae was called ‘the eminent Danish antiquary’ (I, 21) and Thomsen’s method provided ‘the foundation for Archaeology as a science’ (I, 23). Wilson clearly believed that the system of arranging Nordic antiquities was ‘also applicable to those of Britain’ (I, 23). And after Worsaae visited Edinburgh and reorganized the collections of the Society of Antiquaries (Kehoe 1998: 16), it is no surprise that the structure of Wilson’s *Prehistoric Annals of Scotland* closely reflected the three-age system—which he added with a fourth part for the Christian period. The three-age system thus supplied Wilson with a scheme intelligible enough to classify relics from prehistoric handaxes to medieval torture instruments. He favourably cited Nilsson’s work on the Nordic Bronze Age, but did not consider his broader evolutionist and ethnographic premises. It was the early Scandinavian archaeologists who supplied the soil in which Wilson planted his own research.

Despite the different use of the three-age system, Nilsson and Wilson concurred in reproducing its inherent dualism which set the Stone Age apart from the metal ages. Nilsson agreed with Thomsen and Worsaae on the universality of the Stone Age: ‘Every nation, even those most anciently civilised, has had its Stone Age’ (1868: 191). And Wilson literally named the Stone Age a ‘primeval period’ through which ‘most, perhaps all nations have passed’ (1851: I, 41). Wilson forcefully defended this dualism. According to him, the subject matter of the Stone Age was ‘the history, not of men, but of man; not of nations but of the race’ (I, 1). Later ages, on the other hand, were characterized by successive migrations and showed the result of ‘hardy colonists’ (I, 11). In this stage, progress was no longer an independent development, but brought about by external influences of ‘primitive colonists of Europe corresponding to successive stages of advancement in civilisation’ (I, 13). Nilsson’s work shows the same discrepancy. After his developmentalist work on the prehistoric hunters and fishers in Scandinavia (1838–1843), his essay on the origin of the Nordic Bronze Age took a very different perspective (1863). Studying the petrographs from Kivik in South Sweden, he argued that these resulted from an ancient sun cult which had been imported by Phoenician merchants trading bronze for Baltic amber. Now that Nilsson was dealing with the metal ages, evolutionism was replaced by diffusionism; cultural change was no longer explained in terms of gradual progress but migrating merchants; and universal growth had been substituted by historical connection. This dualism would inevitably affect the use of ethnographic analogies.

Comparative ethnography, folklore and ‘the parallax of man’

Compared to the Danish antiquaries, both Nilsson and Wilson were in favour of a greater contribution of ethnography, though for different reasons. In the case of Nilsson, his biological background proved decisive. Though much of his language resonates Enlightenment philosophy, he was more directly influenced by the zoology of Georges Cuvier, the founding father of palaeontology. Cuvier had developed the method of comparative anatomy on the maxim that understanding an extinct species required comparison of its skeletal morphology with that of present animals. Cuvier was a master in such reasoning; he could reconstruct the entire skeleton of an ‘antediluvian’ species on the basis of a single bone alone. Nilsson’s ‘comparative ethnography’—the term was given in the title of his work (1838–1834)—was an expansion of this comparative anatomy to the cultural realm.⁸ He literally said that in his archaeological work he had at hand ‘the comparative method of instruction, which, under the guidance of the illustrious Baron Cuvier, had been adopted in works on zoology’ (1868: xlviii):

If natural philosophy has been able to seek out in the earth and to discover the fragments of an animal kingdom, which perished long before man’s appearance in the world, and, by comparing the same with existing organisms, to place them before us almost in a living state, then also ought this science to be able, by avail-ing itself of the same comparative method, to collect the remains of human races long since passed away, and of the works which they have left behind, to draw a parallel between them and similar ones, which still exist on earth, and thus cut out a way to the knowledge of circumstances which have been, by comparing them with those which still exist. It is by following this method that we shall begin to investigate this subject. (lx-lxi)

The point could not be clearer: like in comparative anatomy, the comparative ethnographer was to juxtapose his Stone Age artefacts ‘with similar objects still existing and still in use’ (lx).

Wilson’s enthusiasm for ethnography emerged in a different context. Initially, the role of analogy was as narrow as it had been with Thomsen and Worsaae, i.e. that of illustrating Stone Age tools. In his first work, his advice for redesigning the British Museum echoed the displays of the Copenhagen museum:

Were an entire quadrangular range of apartments in the British Museum devoted to a continuous systematic arrangement, the visitor should pass from the ethnographic rooms, showing man as he is still found in the primitive savage state, and destitute of the metallurgic arts; thence to the relics of the Stone Period, not of Britain or Europe only, but also of Asia, Africa, and America, including the remarkable primitive traces which even Egypt discloses. (1851: I, xxv)

Yet after his move to North America—Wilson was appointed professor of history in Toronto in 1853 (Kehoe 1991)—ethnography became the loudly heralded cornerstone of his investigations. This move to the New World, though some-

⁸ Quite independently, Hegardt (1996) has reached a similar conclusion; see also the work of Regnéll (1983).

times experienced as an intellectual exile, widened his archaeological vision. In *Prehistoric Man* from 1862, Wilson reflected on this new context in a somewhat awkward third person account:

The author had already familiarized himself with the unwritten chronicles of Europe's infancy and youth, when unexpectedly transplanted among the colonists of another continent, and within reach of aboriginal tribes of the American forests. "The eye sees what it brings the power to see;" and in these he discovered objects of interest on many grounds, but chiefly from the fact that he soon perceived he had already realized much in relation to a long obliterated past of Britain's and Europe's infancy, which was here reproduced in living reality before his eyes. (1862: xxiii).

Wilson was perhaps the first since Lafitau who so explicitly claimed that a study of American natives could enhance the understanding of Europe's past, because like the French Jesuit he had access to first-hand ethnographic evidence. Time and again he stressed how 'America has much to disclose in illustration of primitive history' (14), a statement which reverberates Locke's adage, 'In the beginning all the world was *America*'.

The contribution of ethnography, however, remained generally limited to Stone Age studies. This is very clear from comparing Nilsson's essay on hunters and fishers with his work on the Bronze Age. In the first, he endeavoured 'by a new method, to gain a knowledge on the first inhabitants of the Scandinavian Peninsula' (1868: lvii) and this consisted of enquiring 'whether similar implements are still in use amongst savage tribes now living' (2). But in the latter, he abandoned comparative ethnography and turned to a study of European folklore. For more recent periods, he reasoned, much more could be learnt from the traditions still prevalent on the European countryside than from exotic customs. He thus interpreted the sun worship of the Phoenicians by reference to certain fire rituals in contemporary Ireland, believing that these were remnants of pagan times. Nilsson wrote: '*Aus diesem Gesichtspunkt ist die Untersuchung des Volksaberglaubens für den Ethnographen von Wichtigkeit, um die älteste Geschichte des Stammes zu entwirren*' (Nilsson 1863: 32). The interest for modern superstitions and irrational traditions as clues for European prehistory is an early example of what Tylor eventually called 'the study of survivals'.

The dualism between the Stone Age and the metal ages came down to Nilsson's methodological contrast between comparative ethnography and folklore studies. Ethnography could be invoked as long as the Stone Age was considered a period 'out of time' where historical phenomena had no hold. The Stone Age thus resembled the primeval *état de la nature* postulated by eighteenth-century scholars like Rousseau. It showed humanity in its most natural condition, while the later phases of prehistory exemplified mankind in different cultural traditions. Wilson agreed wholeheartedly: ethnography was especially helpful for the oldest, most natural periods. Since his prime aim was 'to view Man, as far as possible unaffected by those modifying influences which accompany the development of nations',

he turned without hesitation to ‘the Red-Man, indigenous, seemingly aboriginal, and still in what is customary to call a state of nature’ (1862: xix, 4).

Yet what was the purpose of these ethnographic analogies? In fact, an interesting development can be seen. Originally, ethnography was simply invoked to illustrate Stone Age implements. This was the case with Wilson’s suggestions for the British Museum, it was the method favoured by Thomsen and Worsaae, and it was also supported by Nilsson’s comparative ethnography. In his catalogue of Scandinavian artefacts, Nilsson gave first descriptions (distilled from reports by travellers and missionaries) of the tools used by savages, followed by presentations of archaeological specimens. Wilson drew similar ethnographic parallels for prehistoric stone tools, canoes, querns and megaliths. Scottish flint knives were compared with the ones found in the Mississippi valley mounds; stone axes with specimens from the South Sea islands and the American north-west coast; perforated stone tools with the ones fabricated by the Shoshone Indians; and wood combs with their Inuit equivalents. Mostly these parallels were simply mentioned. Juxtaposing prehistoric and present tools was believed to elucidate somehow their fabrication and function. Sometimes a more explicit inference was drawn. This was the case for the small, enigmatic stone cups Wilson had encountered in Scottish collections which presented a ‘striking analogy’ to a class of stone vessels still in use in the Faroe Islands as oil lamps (1851: I, 208). Wilson believed that, considering the similarity of form and decoration and the proximity of the Faroe Islands to Scotland, the prehistoric specimen must also have been used as oil lamps, although perhaps more in ‘mysterious rites of the so-called Druidical temples’ (I, 209) than in mundane domestic contexts (figure 4).

Later, however, ethnographic analogy started to address much broader issues, such as the extent of primitive culture or the degree of civilization. Nilsson regretted that ‘not even one of the savage nations now living has yet been studied or described from a truly scientific, that is to say, from a comparative ethnological point of view’ (1868: 3) so that his study was limited to tools. Wilson, however, once in North-America, could observe ‘an interesting phase of primitive social life’ (1862: 21). According to him, the American natives were ‘a parallax of man, already viewed in Europe’s prehistoric dawn’ (xxiii) and represented ‘man in the initial stages of savage life’ (1). A fuller understanding of contemporary savagery held many promises for prehistoric reconstruction beyond technology, but to draw the implications of such awareness was quite a step further than interpreting oil lamps. Wilson started his chapter on the European Palaeolithic with the remark that American Indians supplied ‘very significant analogies to recently discovered works of art of the cave-breccias and the drift’ (22), but refrained from drawing any precise inferences. Despite the enthusiasm, Wilson’s *Prehistoric Man* is therefore rather disappointing in terms of elaborate ethnographical parallels.⁹ Though Wilson did not quite realize his ambition, the idea that ethnographic analogy could do more than explain stone tools had nonetheless been set. And would be developed by the evolutionists after him.

9 The promise of ethnographic parallels had also a rhetorical function. Presenting his compendium of American artefacts as ‘researches into the origin of civilisation in the Old and the New World’, as Wilson’s subtitle read, was perhaps a way of drawing European attention to the work of an exiled researcher in North-America.

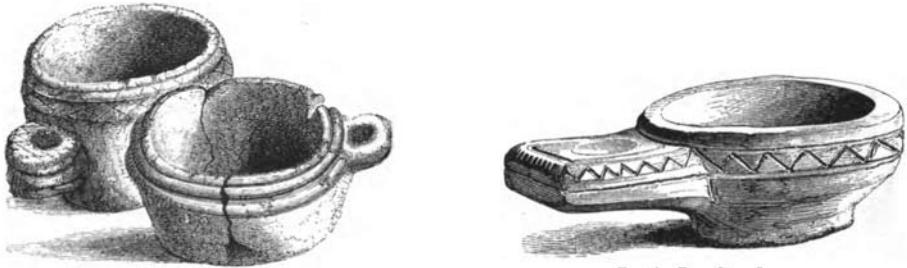


Figure 4. Wilson compared stone vessels (left) found in Scotland with oil lamps still in use on the Faroe Islands (right). General shape and decoration were believed to be sufficiently similar to infer an analogous function. His piecemeal use of analogy on the basis of formal similarity was exemplary for the first half of the nineteenth century (Wilson 1851: I, 207, 208)

The quality of the more piecemeal analogies depended on the similarity between source and target. Hence the insistence on resemblance. Wilson stressed the likenesses between Faroer and Scottish oil lamps. According to Nilsson (1868), one tool had to be ‘exactly like’ another (28), ‘perfectly similar’ (101), or ‘of the very same kind’ (100), or ‘surprisingly similar [...] in the most minute details’ (104). It seemed sometimes as if two tools ‘had been made by the same hand and on the same day’ (104). Nilsson’s analogies worked from ‘the similarity, or rather identity’ (l) between present and prehistoric tools. The most elaborate example was his comparison of Scandinavian passage-graves with Eskimo dwelling-houses. He pointed to the ‘most surprising similarity’ (132) and ‘unmistakeable [sic] resemblance’ (141) of the monuments which were ‘identical in all essentials’ (134): ‘One cannot but be astonished, when reading the description of our Scandinavian gallery-graves, to find it applicable, almost word for word, to the Greenland huts’ (135).

But what did this similarity mean? Did it suggest that implements or monuments had belonged to one and the same tribe? Nilsson said: ‘We must, after a strict examination, answer *No*; they only indicate the same degree of civilisation’ (103). Technology was an indicator of overall progress; technological similarity meant moral or intellectual similarity so that ‘the people who, in Scandinavia, made use of similar implements, stood in the same low degree of civilisation as these savages’ (100; cf. 2). Since there was ‘not the least sign’ of Inuits inhabiting Scandinavia, ‘the similarity [between graves and houses] must be ascribed to the fact that they were in the same grade of civilisation’ (141). Formal similarity determined the quality of the analogy and indicated the broader degree of civilization. Wilson (1851; 1862), however, took a different perspective: according to him, similarities of form generally indicated migration, invasion or diffusion, and he assembled historical, archaeological and ethnographic data to show that such long-distance travels had been possible by the use of canoes.

Yet the question remained for both: why did savage tribes, so diverse in time and space, sometimes come up with such similar tool kits? They must do so ‘instinctively, and in consequence of a sort of natural necessity’, Nilsson suggested (104). To him, at least,

this revealed ‘the evidence of a higher Wisdom’ (105). Wilson, too, agreed that global similarity was explained by ‘some cause operating naturally at a certain stage of development in the human mind’ (1851: I, 147–8), which was related to his view of the history of civilization as ‘successive ideas of the Divine Creator thought out into a recorded actuality’ (II, 528). It would lead too far to discuss early nineteenth-century definitions of Divinity, but it should be kept in mind that the notions of ‘higher Wisdom’ and ‘Divine Creator’ used by Nilsson and Wilson are not necessarily biblical, but may be spiritualist, rationalist, or Aristotelian.¹⁰ The important point to note is that they reasoned from similarity and this was explained in terms of degree of civilization, migration, and divine actualization.

An important device

In the wake of the three-age system and before the establishment of human antiquity, archaeological ideas were not framed by one consistent paradigm. Enlightenment ideas on growth, progress and ‘man in the natural state’ went hand in hand with notions reminiscent of the theological tradition like the unity of the human race, the dispersion of the human family and divine revelation, as well as with a Romanticist interest in national history. Classical texts were still a major source of inspiration for explaining the more recent invasions, trade networks and migrations. To the modern eye, the pages of Nilsson’s and Wilson’s books may show contradictions between invading races on the one hand and the evolving human race on the other, but the fact that it was not experienced as such by the authors says something of the dualist perspective in which they worked.

Despite obvious differences, Nilsson and Wilson paralleled each other in a number of respects. Both the rationalist zoologist and the romanticist antiquary were acquainted with the three-age system of the Copenhagen antiquaries (though Wilson’s enthusiasm was greater); both had come to it from an external point of view (zoology in Lund; antiquarianism in Edinburgh); both extrapolated it to another region (Sweden and Scotland); both put the Stone Age apart from the other periods (as a universal epoch); both treated this epoch in evolutionist terms (as opposed to diffusionist terms for the later periods); both injected the study of this earliest period with ethnography; and both regarded global similarities as signs of divine action. Nilsson’s stay in Paris where he worked under Cuvier had been decisive in formulating his comparative ethnography; so had Wilson’s stay in Toronto where he could observe American Indians *in vivo*. Yet a previous acquaintance with Thomsen’s notion of a Stone Age was essential. Their views can be compared with that of their contemporaries who had not been exposed to the three-age system. To take the most famous example: to the young naturalist Darwin whose early interests were more in biology and geology than antiquarianism, the inhabitants of Tierra del Fuego which he visited in the 1830s did not once remind him of some ancestral relics. This idea only surfaced in the

10 Wilson’s ‘recorded actuality’ echoes the Aristotelian concept of God whose creation is an actualized version of his potential. Wilson worked, however, with the temporalized version of this scheme as we know it since the late eighteenth century, like in Hegel’s phenomenology (Lovejoy 1936).

Descent of Man, written forty years later in the midst of the socioevolutionist climax (1871: II, 404). Unaware of the idea of a universal Stone Age, young Darwin did not yet think of ethnographic analogues.

In the work of these two rather ‘marginal’ scholars (in geographical and disciplinary respect), ethnographic analogy had become an important device for illustrating and interpreting stone tools. Though Wilson tried to broaden the application, wholesale projections were still not made. Before 1860, the anthropological significance of contemporary savagery ‘tended to be of primarily descriptive antiquarian or humanitarian interest’ (Stocking 1974a: 414). Source analogues could be simply picked out in a piecemeal fashion since modern Stone Age people belonged to the homogeneous substrate of savageness.

The analogies drawn rested on one and the same principle, i.e. that of similarity. Nilsson (1868: 103) was inspired by ‘the great resemblance that exists amongst the stone implements of nations of different tribes, during very different periods and in the most distant countries of the earth.’ And Wilson wrote: ‘There are modern as well as ancient prehistoric races; and both are available for solving the problem of man’s true natural condition’ (1862: I, 3). Stone Age people in the present did not just *resemble* the ones from the past, they *equalled* them. They were instances of the same natural kind: savagery. Modern examples of it, unaffected by civilization, did literally ‘re-present’ the past, i.e. they showed the past in the present. In logical terms, the American Indians, the inhabitants of the South Sea Islands, and the ‘Esquimaux’ were sources believed to be identical to the target. It sufficed to simply juxtapose present and past implements in order to explain them. A small amount of similarity, mostly in tool form, was considered sufficient for inferring the degree of civilization.

If the Stone Age was accepted as a world-wide phenomenon, it should not be forgotten that in Britain by the middle of the nineteenth century its time depth was still very shallow, comprising at maximum some thousands of years. Another prerequisite for the emergence of ‘social evolutionism’ demanded the unambiguous establishment of a high human antiquity. A year before Lyell’s famous *coup de grâce* to the short chronology appeared, Wilson had written: ‘Time is the element most frequently required in the hypotheses of the ethnologist’ (1862: I, 111). The ethnologist was given it, more than generously.

The antiquity of man and early social evolutionism

In contrast to Nilsson and Wilson, thinkers like Tylor, Spencer, Morgan, Lubbock, Pitt Rivers, Maine, and McLennan have received more scholarly interest from historians of science (Murphree 1961; Burrow 1966; Harris 1968: 142-216; Nisbet 1969: 159-208; Mandelbaum 1971: 93-111; Stocking 1974a; 1987; Ingold 1986; Kuper 1988; Bowler 1988: 133-41; 1989: 30-9; 1990: 190-201; Sanderson 1990: 10-35). Regarded as the epitomes of social evolutionism between 1860 and 1880, these authors are generally treated as closely related members of the same family who have a lot in common: exponents of the Victorian epoch firmly rooted in the London scientific circles (Morgan excepted) after the publication of Darwin’s

Origin of Species, most of them shared the beliefs in the psychic unity of mankind (as against the polygenist school), in overall progress, in the slow, gradualist, uniform and law-like nature of this progress, in the unilinear trajectory of progressive growth, in independent invention as the basic mechanism of this growth, in the differential extent of progress among contemporary societies, in the possibility of arranging these societies in a hierarchical sequence from simple to complex and from savage to civilized, in technological attainment as the first criterion for establishing such hierarchy, in the comparative method as a means of attesting previous stages of an advanced society, and in the study of survivals as a verification of the ladder of sociocultural complexity. These indeed can be called the basic tenets of classical evolutionism (cf. Stocking 1974a: 409; 1987: 170).

However, if it were all that simple there would not have been such elaborate scholarship on the theme. The above sketch is a caricature based on the most classical evolutionist writings like Tylor's *Primitive Culture* (1871), Morgan's *Ancient Society* (1877) and Spencer's *Principles of Sociology* (1876-1896).¹¹ Many authors diverged from such an ideal-typical portrayal.¹² This holds especially true for the comparative method. This methodological pivot of Victorian social evolutionism has often been treated as a single, unified principle, illustrated by a few hackneyed quotes from Lubbock and Tylor. This is unfortunate since there was a lot of variety and change through time. In this and the following section, I shall not confine myself to the few theoretical lines in evolutionist writings. Drawing attention to the actual applications of comparative reasoning will allow a better understanding of the development of the comparative method.

The first generation of social evolutionists

It is now generally acknowledged that not the publication of Darwin's *Origin of Species* in 1859, but the establishment of human antiquity triggered the sudden emergence of social evolutionist thought in Britain. Though authors like Lubbock and Spencer were in touch with the naturalist science of Darwin and though the evolutionists sided themselves with the Darwinian revolution and its rejection of religious dogmatism, intellectually, social evolutionism developed independently from Darwinian principles (Murphree 1961; Ingold 1986: 14; Bowler 1988: 134-5; 1989: 34; 1990: 191). Nuclear tenets of Darwin's theory (such as random variation, selective pressure, differential survival, non-linear change, minimal role of teleology) are totally absent in the works of Lubbock, Tylor, Morgan and all the others.¹³ Instead, the principle of unilinear progress, the idea of directional laws (Mandelbaum 1971: 111), and the belief in immanent, necessary and natural change (Nisbet 1969: 166-88) we find with the evolutionists is much more in line with Lamarckian orthogenesis and eighteenth-century developmentalism than with

11 Sanderson (1990) presents a recent example of such schematic view of social evolutionism.

12 Stocking's *Victorian Anthropology* (1987) was first of all an attempt to show the diversity of contemporary social thought (cf. Stocking 1974a). In his discussion of individual authors, Stocking constantly seems to have the prototype of social evolutionism in mind as a yardstick against which their contributions are measured.

13 Recently, Sanderson (1990: 30) has argued that Tylor and Morgan both had 'a loose kind of natural-selection conception'. The evidence for this is thin. Bowler (1988: 138) was probably much closer to the truth when he compared evolutionism with the 'anti-Darwinian theory of orthogenesis'.

Darwinian ‘descent with modification’ (Stocking 1987: 178; Bowler 1988: 139).¹⁴ It is, therefore, better to speak of *Darwinians* than of *Darwinists* to indicate the group of loyal but idiosyncratic followers, and to indicate their theory as *evolutionist* rather than *evolutionary* (Sanderson 1990: 3).

Not the writings from an old man in Kent but the discoveries of some young geologists in Devon made the evolutionist wheel spin. This is the conclusion reached by several historians of science after studying the impact of the excavations at Brixham Cave where proof was obtained for humanity’s existence in geological times (Gruber 1965; Grayson 1983; Trautmann 1992; Van Riper 1993). The establishment of human antiquity was experienced by its contemporaries not only as a sudden revolution in geology, but also in palaeontology, prehistoric archaeology and anthropology (Van Riper 1993: 192-221).¹⁵ In the first edition of *Prehistoric Man* (1862), Wilson included a chapter simply called ‘Guesses at the Age of Man’; the second edition (1865) already spoke firmly in favour of ‘the traces of a period irreconcilable with any received system of historic chronology’ (29). Next to a new edition of Wilson’s work, the year 1865 saw the publication of three highly influential books: Lubbock’s *Pre-Historic Times*, Tylor’s *Early History of Mankind* and McLennan’s *Primitive Marriage*; three volumes which can be rightly seen as laying the foundations of prehistoric archaeology, sociocultural anthropology and comparative law respectively. All three were heavily indebted to the enormous expansion of human chronology. Besides the long chapters which Lubbock spent paraphrasing Lyell, time was the key feature around which all the explanations of technological development, of moral and intellectual progress, of the growth of social institutions and historical customs pivoted: the Enlightenment idea of progress could now finally be considered from a long term perspective. There was, therefore, an intimate connection between the high antiquity of humanity and the low condition it once had: ‘In reaching a time indefinitely more remote,’ McLennan (1869: 523) said, ‘we have come on a condition of man indefinitely lower.’

Apart from time, geology also provided a method and a metaphor for thinking about social evolution, to the extent that the nascent field of prehistoric archaeology has even been entitled ‘geological archaeology’ (Van Riper 1993: 185).¹⁶ ‘The archaeologist can only follow the methods which have been so successfully pursued in geology,’ Lubbock

14 For an alternative reading, see Weber (1974) who holds, rather idiosyncratically, that social evolutionism was more immediately influenced by early nineteenth-century biology than by eighteenth-century philosophy. Even if this may have been a source upon which the evolutionists drew, in the end their conclusions resembled still more the Enlightenment tradition. Stocking (1987: 178) has rightly said: ‘In filling the void in cultural time with the data of contemporary savagery, they were carried back into close contact with an earlier developmental tradition, to which their own sociocultural evolutionism was in many respects closer than it was to Darwinism.’ And Nisbet went so far as to state: ‘The theory of social evolution is no more than the eighteenth-century theory of natural history—broadened, extended, ramified, and filled with a volume of ethnographic data not known to such men as Ferguson, Smith, and Rousseau’ (1969: 165).

15 In fact, it had been the result of a long process but the final blow was often represented as the only one.

16 He thus set it apart from ‘historical archaeology’ which dealt with the periods still referred to by classical texts. The study of prehistory lay at first in ‘a no-man’s-land between geology and historical archaeology’ (Van Riper 1993: 192). This is where the geological archaeologists would pick it up. Lubbock (1865: 2), the unofficial spokesperson of them, stated: ‘Archaeology forms, in fact, the link between geology and history.’

wrote (1865: 339). In view of such a bottomless past, the authority of classical sources decreased. Tylor criticized the ‘undue confidence in the statements of ancient writers’ (1865: 2). And for McLennan philologists had to be contented ‘to act as assistants rather than as principals’ (1865: 7). Other sources comparable to direct geological evidence were now to be invoked. The law historian Maine (1861: 3) held that ‘rudimentary ideas are to the jurist what the primary crusts of the earth are to the geologist.’ And in a famous phrase, Lubbock (1865: 336) said that present savages ‘are to the antiquary, what the opossum and the sloth are to the geologist’.

The crucial books published in 1865 each stressed the importance of ethnography. After ten chapters on European prehistory and the antiquity of man, Lubbock’s influential *Pre-Historic Times* continued with three chapters on ‘modern savages’, meant ‘to throw some light on the remains of savage life in ages long gone by’ (1865: viii). He entirely concurred with Nilsson’s derivation of comparative ethnography from comparative anatomy, ‘the rude bone- and stone-implements of bygone ages being to the one, what the remains of extinct animals are to the other’ (336). Tylor’s *Researches into the Early History of Mankind* started from the premise that present-day civilization was not intelligible without considering its historical development, but since much material lay ‘out of the beaten track of history’, one needed ‘indirect evidence’ (1865: 4). Tylor thus based his researches on the available ethnographic literature on ‘the lower races up and down in the world’ (1). For ‘matters of practical life’ contemporary savages ‘may be nothing to us’, he said, but reading about them helped ‘completing the picture, and tracing out the course of life’ (2). In *Ancient Law*, the jurist McLennan criticized the philological tradition of Maine and Bachofen for dealing with the origin of social and legal institutions (Lowie 1937: 39–54). ‘The facts disclosed by philology [...] cannot be said to tell us anything of the origin or early progress of civilization,’ he held, ‘for the features of primitive life, we must look [to the tribes] of Central Africa, the wilds of America, the hills of India, and the islands of the Pacific’ (McLennan 1865: 5, 6).

All three authors accepted the unity of the human race and strongly believed in progress, although for Lubbock and Tylor the underlying mechanism could very well involve migration. Following Nilsson’s dualism, Lubbock said that migrations were ‘compatible only with a comparatively high state of organisation’ (1865: 476) and Tylor believed that explanations by independent invention had ‘no historical value’, while the ones based on diffusion and migration had this ‘in a high degree’ (1865: 5). Only McLennan supported the evolutionist tenet of independent invention. Clearly, this divergence would influence the structure of analogical reasoning. With their archaeological interests, Lubbock and Tylor evidently supported the three-age system, the sequence now being attested ‘in almost every district of the habitable globe’ (Tylor 1865: 4). After the establishment of human antiquity, the Stone Age was not only universal but also extremely long. The road to more elaborate uses of the comparative method lay now open.

The function of contemporary savagery

For the first generation of social evolutionists, the use of ethnographic parallels was diverse and the logic of analogy was multifarious and often muddled. Reading through the 1865 monographs of Lubbock, Tylor and McLennan, one comes across a variety of purposes, strategies and applications which shows how the comparative method was far from being standardized at that stage.

Illustrating and explaining tools

The straightforward juxtaposition of prehistoric tools or monuments with recent specimens remained a popular approach. Lubbock, in line with Nilsson, defended this strategy:

If we wish clearly to understand the antiquities of Europe, we must compare them with the rude implements and weapons still, or until lately, used by savage races in other parts of the world. (1865: 336)

In *Pre-Historic Times*, Neolithic tumuli were compared to ‘the burial mound of Oberea, in Otaheiti’ to show that people without metallurgical knowledge could erect such constructions (1865: 110). The Swiss and Irish lake-dwellings were flanked by a set of ethnographic parallels (122). Prehistoric burials of a woman with an infant became more intelligible after an exposition on the ‘Esquimaux’ habit of burying their babies alive when the mother had deceased (116). The Inuit were also invoked to show that eating foxes was not limited to the Swiss lake-inhabitants (141), nor that lingering faunal remains was restricted to cave-dwellers in the Dordogne (256). The formation of Danish kjøkkenmøddings was similar to modern shell-middens in Australia, the Malay Peninsula and especially Tierra del Fuego (178). Indeed, Danish mound-dwellers must ‘have lived in very much the same manner as the Tierra del Fuegians, who dwell on the coast, feed principally on shell-fish, and have the dog as their only domestic animal’ (189). The function of such ethnographic parallels was nothing more than just illustrative. Savagery being an a-temporal category, it could both be ‘illustrated by ancient remains and the manners and customs of modern savages,’ as Lubbock’s subtitle said. Prehistory was found in the soil of Europe and the soul of the savage.

However, on certain occasions the use of ethnography led to a genuine analogy. This was especially the case for attributing functions to tools. Wondering what the function of Palaeolithic scrapers could be, Lubbock compared them with ‘Esquimaux’ tools which were used as skin-scrapers. ‘The true nature and use of the ancient skin-scrapers has, however, been entirely explained by these modern specimens with which they are absolutely identical,’ he concluded and added in good Victorian fashion that ‘the method of preparing skins is curious and ingenious, but very disgusting’ (407, cf. 71). Like with Wilson’s oil lamp, one attribute of the present source (the function of the Inuit tool) was transferred to the past target (the Palaeolithic tool) on the basis of shared

—	EASTER ISLANDERS.	FUEGIANS.	BUSHMEN.	HOTTENTOTS.	ANDAMANERS.	AUSTRALIANS.	ESQUIMAUX.	NORTH AMERICAN INDIANS.		NEW ZEALANDERS.	FEEGEANS.	SOCIETY ISLANDERS.	PACIFIC ISLANDERS.		
								North-East.	West.						
Bows and Arrows	...	Weak	Weak	Weak	Good	Good	...	Good	Good	...	Good	Weak	Weak
Slings	...	Yes	Yes	...	Yes	Yes	...	Yes	Yes	?
Throwing Sticks	Yes	Yes	Yes	?
Boomerangs	Yes	Yes
Bolas	Yes	?
Pottery	Yes	Yes	...	Yes
Canoes	Bad	Bad	Good	...	Bad	Good	...	Bad	Middling	Very good	Very good	Very good	Very good
Agriculture	Maize	Yes	Yes	Yes	Yes
Fortifications	Many	Yes	...	Yes	Yes
Fish-hooks	...	Stone	...	Iron	?	Neat	Bone	...	Yes	Yes	Bone & shell	Bone & shell	Bone & shell	Shell	Shell
Nets	Yes	Good	...	Neat	Small	For bird catching	Yes	Yes	Large	Yes	Large	Yes
Dogs	...	For hunting	For hunting	For hunting	...	For hunting	For draught	For draught	For draught	For hunting	For food	For food	For food	For food	...
Hogs (Domestic)	Some	Maui

Figure 5. Lubbock's compilation of ethnographic evidence served to establish a common denominator of contemporary savagery. While indicating a hierarchy from simple to complex, it also showed that what was commonly shared had to be old (Lubbock 1865: 447)

resemblances (the identical form). Observed formal similarity thus led to a predicted functional similarity.¹⁷

Finding a common denominator

Illustrating tools and explaining their function had already been functions of ethnographic analogy before. But Lubbock went one step further. *Pre-Historic Times* contained more than one hundred pages describing fourteen societies of 'non-metallic savages' (337). This ethnographic compendium was based on an armchair reading of travellers' accounts; it presented a *tour d'horizon* of what Lubbock saw as the world's lowest primitiveness, depicting with much juicy detail and moral judgement the variation of savage life. Lubbock believed that the contemporary wilds represented a general, if heterogeneous, state of savagery which could be arranged from simple to complex. He presented a table of technological progress (figure 5) reached by different savages which resulted in a hierarchy from least advanced (Easter Islanders, Fuegians, Bushmen, Hottentots and Andamaners) over tolerably advanced (Australians, Esquimaux and North-American Indians) to well advanced (New Zealanders, Feegeans and other Pacific tribes).

17 Similar reasonings can be found in John Evans' *The Ancient Stone Implements, Weapons, and Ornaments of Great Britain* (1872). Evans, once involved with the recognition of human antiquity and the rehabilitation of Boucher de Perthes, used ethnographic analogies to infer the function of prehistoric stone tools. Less sweeping than Lubbock, his use of ethnographic evidence was more confined to solving localized problems. He thus focused 'less on the workings of Stone Age Europeans' minds than on the works of their hands' (Van Riper 1993: 211).

Though not always clear, Lubbock's interest was in finding the basic commonalities of savage life. Because spears and clubs were the two tool types which occurred among all contemporary savages and because such useful tools would not be rapidly lost in the course of civilization, 'they seem to be the only natural and universal weapons of man' (475). The more geographically spread an implement was, the more primeval it seemed. The argument was not limited to the technological realm. Lubbock reasoned that because in general savages were cruel, childlike and unfair to women and because most of them could not count up to ten, trusted in witchcraft or even lacked a religious concept, this moral and mental inferiority was also projected into the past. For example: 'That our earliest ancestors could have counted to ten is very improbable, considering that so many races now in existence cannot get beyond four' (475). Though obsessed with the extremes of savagery, Lubbock sought a common denominator between several forms of savage life in order to find the primary technological, moral and mental conditions.

Drawing developmental sequences

Establishing a hierarchy could also serve a different purpose because it allowed to draw a developmental sequence. This is how Tylor worked. Arranging ethnographic, historical and archaeological evidence on a specific custom from simple to complex, he sought to establish conjectural sequences of cultural growth. This is most clearly seen in Tylor's discussion on the origin and development of the use of fire (figure 6). Tylor started with 'stories of fireless men in America' (232) whose authenticity he suspected; he continued with 'a kind of transitional state' of fire-using without fire-making, a condition which until recently would have been found among the Tasmanians; he then proceeded to a discussion of the art of making fire 'between the rudest and most artificial way in which this may be done' (236): first, there was the 'stick-and-groove' technique, still found in the South Sea Islands; then came the manual 'fire-drill', observable among Australians, Mexicans, Veddahs and many other contemporary nations; this was followed by a cord-drill, an implement still in use among the 'Esquimaux', later by a bow-drill (present with the North-American Indians) and finally the pump-drill, found among the Iroquois Indians and among English 'china and glass menders'. Every stage of this technological pedigree was illustrated with an engraving; it was corroborated by textual and mythological evidence from the writings of Pliny, Russian myths and Finnish poems. The methodological rationale underlying this conjectural sequence was clearly outlined by Tylor:

A survey of the condition of the art in different parts of the world, as known to us by direct evidence, is enough to make it probably that nearly all the different processes found in use are the successors of ruder ones; and, beside this, there is a mass of indirect evidence which fills up some of the shortcomings of history, as it does in the investigation of the Stone Age. (1865: 236)

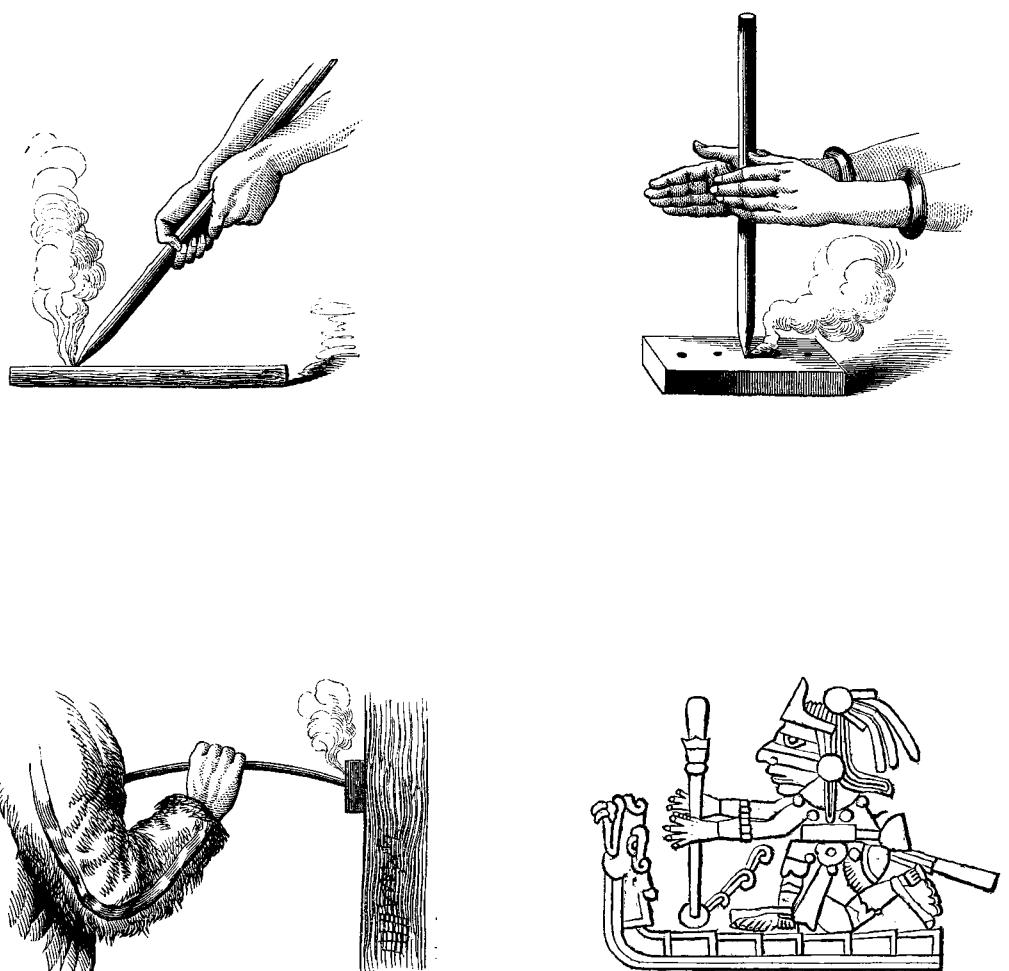
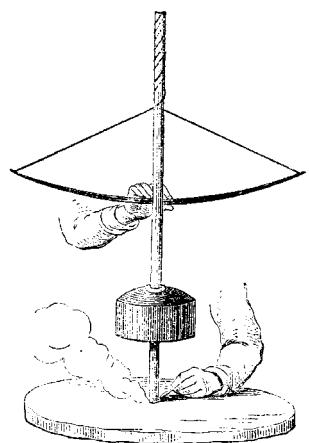
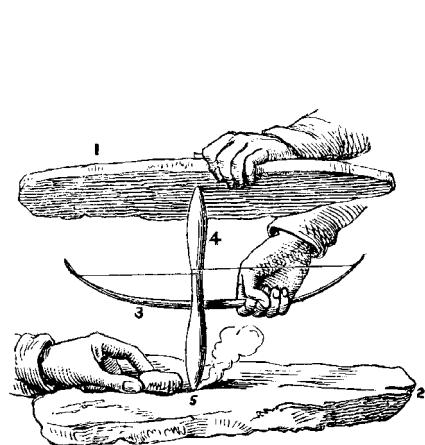
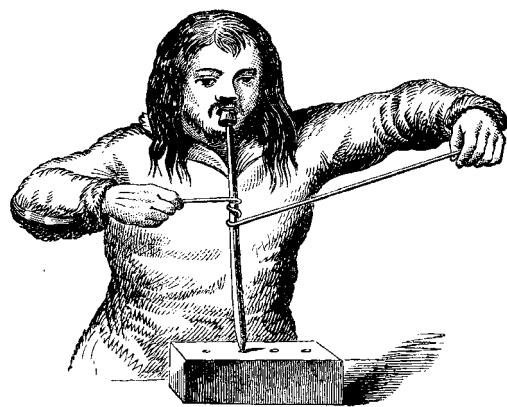


Figure 6. (both pages) Tylor scanned ethnographic and archaeological data to outline a progressive development in fire-making technology. According to him, it had developed from the stick-and-groove technique, over the fire-drill, the cord-drill and the bow-drill to the pump-drill. This sequence, however, was not independently confirmed; it presented an assumed line of technological progress, irrespective of time or place. The ancient Mexicans and the modern Gauchos of the Pampas could therefore be classified in the same level of using the fire-drill (Tylor 1865: figure 20-26, 29)



The history of early civilization could thus be known by a carefully planned and somewhat erratic expedition in Africa, America, Asia and Oceania. It was based on the a priori assumption that the chronological order could be read off of the logical order from simple to complex. So strong was the belief in progress that the sequence drawn by Tylor 'was not an order which had been established by independent chronological evidence, but was a function of the system of classification' (Mandelbaum 1971: 106).

Broader applications

This logic was entirely followed by McLennan, safe that he applied it to bolder themes. Not fire-making but the origin of bride-kidnapping and other marriage ceremonies were addressed by him. Wilson's suggestion that one could move beyond a study of tools received a full application in McLennan's work on the development of social and legal customs. Contemporary ethnographic evidence could do more than explaining technology:

These facts of to-day are, in a sense, the most ancient history. In the sciences of law and society, old means not old in chronology, but in structure: that is most archaic which lies nearest to the beginning of human progress considered as a development, and that is most modern which is farthest removed from that beginning.
[...]

The preface of general history must be compiled from the materials presented by barbarism. Happily, if we may say so, these materials are abundant. So unequally has the species been developed, that almost every conceivable phase of progress may be studied, as somewhere observed and recorded. And thus the philosopher, fenced from mistake, as to the order of development, by the interconnection of the stages and their shading into one another by gentle gradations, may draw a clear and decided outline of the course of human progress in times long antecedent to those to which even philology can make reference. (McLennan 1865: 6)

The unity of the human race, the progressive nature of society, the unilinear trajectory of progress, the unequal development of contemporary societies, the methodology of using one stage of a present society as representative of another's past state, the possibility of thus drawing a general history of mankind: it is all argued here in a clarity of style unknown to Tylor and certainly to Lubbock. Burrow appreciated this argument as 'one of the clearest, most elaborate and least apologetic of all the expositions of the principles of the Evolutionary Comparative Method' (1966: 233) and Stocking's portrayed McLennan as being 'more brilliant than Tylor or Lubbock, and more given to intellectual enthusiasm' (1987: 168). McLennan's notion of independent invention allowed to scan the world for evidence which could fill the gaps of his unilinear progressionist trajectory. Since contemporary societies had 'not advanced in civility *pari passu*' (1865: 9), it was possible to arrange them into a sequence of progressive growth with gentle overlaps.

Ritualized customs

A final means of using contemporary evidence to enhance an understanding of the past was somewhat more peculiar as it entailed information from the civilized world. Stone tools could be adequately explained by exotic specimens, but social and legal history required richer data for comprehension. Both Tylor and McLennan believed, therefore, that ritualized customs in our own society corresponded to older realities. ‘The symbolic forms that appear in a code or in popular customs,’ McLennan said, ‘tell us as certainly of the usages of a people, as the rings in the transverse section of a tree tell of its age’ (1865: 9). According to Tylor, European earrings for instance could not be understood ‘as a product of our own times, but as a relic of a ruder mental condition’, which needed to be compared with the perforated noses from Papua New Guinea, elongated earlobes from East-Africa and mutilated lower lips from Amazonia. Just like Nilsson turned to European folklore, some of the social evolutionists turned to rituals in the modern world to understand prehistory. The study of these petrified customs would later develop into Tylor’s important notion of ‘survivals’ but he gave already a preliminary outline:

I cannot but think that [such examples] are to be explained as being, to use the word in no harsh sense, but according to what seems its proper etymology, cases of superstition, of the “standing over” of old habits into the midst of a new and changed state of things, of the retention of ancient practices for ceremonial purposes, long after they had been superseded for the commonplace uses of ordinary life. (1865: 218, original emphases)

Traces of primeval savagery were not only to be found in the horrific practices of contemporary wilds, but also in the jewellery and earlobes of Victorian young ladies.

Ethnographic enthusiasm

The first generation of social evolutionists followed the enthusiasm for ethnographic analogy of Nilsson and Wilson, but built further upon it. Once restricted to clarifying implements and monuments from the Stone Age through similarities with contemporary cases, now ethnographic analogy was used in a variety of contexts. Lubbock’s search for a common denominator among contemporary savagery, Tylor’s conjectural sequence of developmental growth, and McLennan’s application of it to social and legal contexts, all went beyond the parallels drawn by Nilsson and Wilson. Despite their immediate differences, they betray a changing appreciation of the role of ethnography in early Victorian evolutionism.

First of all, there was an avidity to move beyond the technological realm. This was not just the case with McLennan, but also with Lubbock: his search for a common denominator equally included a comparison of moral and mental capacities among savages across the world. Now that the three-age system was taken-for-granted and human antiquity had been established, more weighty issues about the history of human civilization were to be addressed. But as the geological record

for reconstructing the life of prehistoric savages went ‘no farther than to inform us what food they ate, what weapons they used, and what was the character of their ornaments’ (McLennan 1865: 5), external assistance had to be sought.

A second important trait concerned the notion of hierarchy. Nilsson and Wilson had been treating the evidence of contemporary savagery as homogeneous, but the Victorian social evolutionists organized it by imposing a particular hierarchy. This can be seen from Lubbock’s comparative table which arranged societies from least advanced to well advanced and from Tylor’s developmental sequence organizing ethnographic customs from simple to complex. According to McLennan, primitive modes of life were to be understood in ‘their classification as more or less archaic’ (1865: 8). The greater role ascribed to the idea of progress, made that such hierarchy from simple to complex implied a development from ancient to modern. This inevitably led to an inquiry into the most primitive form of humanity currently living. Together with many of his contemporaries Lubbock posed the question:

Travellers and naturalists have varied a good deal in opinion as to the race of savages which is entitled to the unenviable reputation of being the lowest in scale of civilisation. Cook, Darwin, Fitzroy, and Wallis were decidedly in favor, if I may so say, of the Fuegian; Burchell maintained that the Bushmen are the lowest; D’Urville voted for the Australians and Tasmanians; Dampier thought the Australians “the miserablest people in the world;” Forster said that the people of Mallicollo “bordered the nearest upon the tribe of monkeys;” Owen inclines to the Andamaners; others have supported the North American Root-diggers; and one French writer even insinuates that monkeys are more human than Laplanders. (1865: 445-6)

Lubbock did not speak out in favour of one these savages. Despite his interest in the lowest of the lowest, he continued with the warning that ‘the present habits of savage races are not to be regarded as depending directly on those which characterised the first men’ (446).

This is a third point of convergence among the first generation of evolutionists: the refusal to project one society onto another. Many ethnographic analogies were drawn, but they were almost invariably limited to piecemeal transfers of specific functions, manners, and customs; wholesale substitutions of a prehistoric case by an ethnographic society were explicitly avoided. The Inuit in Lubbock’s work, for instance, were paralleled on some occasions to the Palaeolithic cave inhabitants of the Dordogne, on others to the Mesolithic shell-mound dwellers in Denmark, and on still others to the Neolithic people of the Swiss lake-side settlements.¹⁸ At no point were they projected at a specific stage of human history. Tylor, too, warned in his conclusion that ‘it does not seem likely that any tribe known to modern observers should be anything like a fair representative of primary conditions,’ particularly since ‘the present condition of savage tribes is the complex result of not only a long but an eventful history’ (1865: 369). Lubbock’s reluctance in this matter was also

18 The term ‘Mesolithic’ is of course an anachronism in this context as the concept only emerged by the late nineteenth century.

given in by an awareness that savage habits might be ‘arising from external conditions’ (1865: 446). Diffusion, migration and other forms of cultural contact favoured by Lubbock and Tylor had affected the pristinity of modern savages. It was only in a later phase of Victorian evolutionism that this awareness of individual history would make place for the doctrine of independent invention and the practice of direct projective reasoning.

A fourth point of agreement was in line with Nilsson and Wilson position’s of valuing similarity as the cornerstone of ethnographic analogy. Lubbock was so sure of his functional interpretation of Danish skin-scrappers because they were ‘absolutely identical’ with the Inuit specimens. He also believed that the Fuegians, though ‘among the most miserable specimens of the human race’, were still of especial interest ‘from their probable similarity to those of the ancient Danish shell-mound builders’ (439). Primitive savages did not only resemble prehistoric ones, they also resembled each other, regardless of their individual histories. They all belonged to the same natural kind. Tylor and McLennan were very firm on this. Tylor (1865: 169) wrote: ‘The state of things among the lower tribes which presents itself to the student, is a substantial similarity in knowledge, arts, and customs running through the whole world.’ Unencumbered by particularist notions like migration and diffusion, McLennan forcefully said:

So far as my inquiries into early social phenomena have extended, I have found such similarity, so many correspondences, so much sameness in the forms of life prevailing among the races usually considered distinct, that I have come to regard the ethnological differences of the several families of mankind as of little or no weight compared with what they have in common. (1865: 3)

Similarity was so essential that dissimilarity could be disregarded. Moreover, as monogenists they were already inclined to stress likenesses over differences (Murphree 1961: 282). Again, as with Nilsson and Wilson, if contemporary savages could be used it was not just because they simply resembled the prehistoric inhabitants of Europe, but because they were essentially believed to be the same. Contemporary savagery was an a-temporal category; old meant ‘not old in chronology, but old in structure’.

Degenerationism and classical evolutionism

The heyday of sociocultural evolutionism are popularly depicted as a triumph of scientific inquiry over religious dogma. All too often it is forgotten that one of the main impetuses for its theoretical development was provoked by the writings of some Christian-inspired thinkers (cf. Hodgen 1937; Murphree 1961: 278). Richard Whately, archbishop of Dublin, had already in the 1830s and 50s expressed his doubts as to whether humanity had arisen from savageness. And George D. Campbell, better known as the Duke of Argyll, commented extensively Lubbock’s *Pre-Historic Times*. His writings appeared as four articles in the review *Good Works* in 1868 but were assembled in *Primeval Man: An Examination of some Recent Speculations* (1869). Far from being an archetypical reactionary attitude of Christian orthodoxy to the new scientific creed, this book—and too a lesser extent

Whately's essays—challenged some of the basic assumptions social evolutionists took for granted (Gillespie 1977). The hardening of socioevolutionist thought in the late 1860s into a more radical, unilinear theory of human progress through independent invention was primarily the result of a consorted reaction to the Duke of Argyll's critique.

Degenerationist doubts

In a range of publications the Irish archbishop Richard Whately objected to the idea that humanity had arisen from savagery (Grayson 1983: 217–8). Since the Scripture spoke of a primeval golden age in paradise, early humanity could by no means have been miserable. According to Whately, contemporary savages were therefore examples of post-Adamite degeneration rather than still living primeval relics. He did not deny that societal progress could take place, but if this happened, it was the result of divine revelation. This had been the faith of Western civilization: after the Fall, Adam's descendants had slowly arisen thanks to God's benevolent interventions. Since savages were too immoral, no example would be found of a savage race that had civilized itself without external assistance. Progress was induced from the outside, either by contact with superior neighbouring societies or by divine intervention. In any case, savages could not pull themselves out of the morass of their inferiority.

Many of Whately's ideas reappeared in Argyll's work—the Christian inspiration, the concept of a primeval perfection, the idea of degeneration amongst contemporary savages—though there were important differences as well. The Duke of Argyll was no religious dogmatist. Despite the numerous quotes from the Bible and Augustine which testify to his personal religious convictions, he held that he reached his degenerationist conclusions on strictly scientific grounds, even if they eventually squared nicely with the account of the Adamite Fall. He also 'set little value on the argument of Whately, that as regards the mechanical arts Man can never have risen "unaided"' (Argyll 1869: 198). Instead of a permanent divine revelation, he thought that after the initial endowment of a body and a mind, man basically took care of his own progress. Whately took the Scripture as a literal source of authority for reconstructing humanity's past; Argyll developed a Christian-inspired, critical inquiry of man's origins. Welcoming the nascent field of prehistory, he said that 'it is not open to dispute that the early condition of Mankind is accessible to research' (24). He dissected available theories into their component parts, analysed them, and rejected or accepted them. His careful argumentation and the crystal-clear structure of his critiques show how he mastered 'the art of scientific controversy' (Gillespie 1977) much more serenely than many of his contemporaries.

If the Duke of Argyll disagreed with Whately, he 'set still less value on the arguments of Sir J. Lubbock' (199). *Primeval Man* was first and foremost a lengthy critique on *Pre-Historic Times*: Lubbock is literally on the first and last pages of the work and the entire critique is directed at him. Nevertheless, Lubbock's book was placed in its broader scientific context and the three substantial chapters of Argyll's essay each dealt with the three major debates of the time: the first one was on 'the origin of man' which entailed a

critique of Darwin's *Origin of Species* (1859) and Huxley's *Man's Place in Nature* (1863); the second dealt with 'the antiquity of man', that is, Lyell's *Geological Evidences* (1863); and the final one with 'man's primitive condition', or Lubbock's *Pre-Historic Times* (1865). As to the first two questions, Argyll's opinion can be rapidly summarized: he did not support a simian descent of humanity (contra Huxley) but could very easily accept a high antiquity of man (pro Lyell). He saw 'a gulf practically immeasurable' between man and the other creatures and believed humanity should be accorded a separate class (1869: 74-5). On the other hand, he knew of 'no one moral or religious truth which depends on a short estimate of Man's antiquity' (74, 127). Geological age estimates were reconcilable with biblical chronology because 'thousands of years are as less than seconds in the Creative days' (125).¹⁹

Argyll vehemently disagreed with Lubbock's conclusions that civilization had emerged from savagery and that present-day savages echoed such ancestral condition. Argyll's critiques against this 'Savage-theory' can be grouped into four lines of dissent. Firstly, whereas Argyll acknowledged the technological inferiority of most prehistoric and present savages, he did not proceed to see this as an indication of moral or mental inferiority. Those who were 'ignorant of the industrial arts' were not necessarily 'ignorant of duty or ignorant of God' (132, 133). Lubbock had always assumed that the degree of technological mastery was a barometer of overall degree of development. Argyll questioned this since he saw no causal correlation between the two.

Secondly, it remained to be seen whether primitive technology was really inferior. According to Argyll, there was 'quite as much ingenuity and skill in the manufacture of a knife of flint, as in the manufacture of a knife of iron' (1869: 150). The most momentous discoveries in human civilization like the use of fire and cereal cultivation had occurred in the remote human past. Since 'the noblest discoveries made by Man were made by him in primeval times,' he reasoned, 'Faraday and Wheatstone are but the inventors of ingenious toys' (154). Argyll did not only undermine the vertical relation of causality, he also denied the horizontal relation of similarity: prehistoric technology was no match for primitive technology.

Thirdly, contemporary savages were too wretched to be representative. Argyll opined that the lower races of today were living in a condition quite unlike the one which characterized our progenitors. He saw savages as 'outcasts of the human family' (173) who had been driven out of their original territories into inhospitable environments on the fringes of the earth. Since 'the lowest and rudest tribes in the population of the globe have been found at the farthest extremities of its great Continents' (162-3), the 'uttermost ends of the world' (Gamble 1992) had special importance for Argyll. Regarding the Eskimo, it was 'hardly possible to conceive a life so wretched' (164), while the Fuegians were 'the most degraded among the races of mankind' (167). Primitive as they were, they still had 'all the perfect attributes of humanity, which can be and are developed, the moment they

19 The crux of Argyll's argument came from recently discovered pharaonic wall-paintings in Egypt which showed that racial differences between negroids and Caucasians dated back to at least the third millennium B.C. These forced him to give up one of the following biblical truths: either there were different species of man and there was no need to revise the short chronology, or there was only one species of man but then one needed more time to explain such old racial differences. Argyll chose the second option.

are placed under favourable conditions' (172). Argyll denied that these were good sources for understanding early human history. 'Is it not absurd,' he asked rhetorically (173–4), 'to argue that the condition of these outcasts of the human family can be assumed as representing the aboriginal condition of Man?'

What the Eskimos and Fuegians showed, above all, was that humanity could decay. So, fourthly, the Duke of Argyll reasoned from the 'indisputable fact that Man is capable of Degradation' (155). Close to the protestant notion of human sinfulness, he believed that human corruption, i.e. man's constant tendency to do what 'he ought not to do', was 'as much a fact in the natural history of Man as that he is a Biped without feathers' (188–9). Where Lubbock saw progress, Argyll found degeneration. The former saw devil-worship as a positive step in the growth of religious sentiments, the latter regarded such horrible practice as 'not in favour of the doctrine of a gradual rise, but, on the contrary, of continuous corruption and decline' (190). This was a central, if poorly defended, tenet of Argyll's misanthropic worldview.²⁰ The clash between evolutionists and degenerationists was one between incommensurable views of human nature, resting more on conviction than argumentation. Lubbock had optimistically concluded that future generations 'will better appreciate the beautiful world in which we live, avoid much of that suffering to which we are subject, enjoy many blessings of which we are not yet worthy, and escape many of those temptations which we deplore' (1865: 492). Argyll's conclusion said 'that even in his most civilized condition, [Man] is capable of degradation, that his Knowledge may decay, and that his Religion may be lost' (200).

The historian of science Gillespie (1977) has argued that contemporary critics reacted in three different ways to Argyll: either one took the theory seriously enough to engage with it, or one rejected it as a scientific theory misled by religious prejudice, or one dismissed it as religious dogmatism altogether. He classified Lubbock and Tylor in the second group. Indeed, they regarded Argyll's ideas as a scientific theory resulting from religious bigotry but they nonetheless spent very many pages refuting it. The degenerationist challenge gave them a common enemy and contributed to the fortification of evolutionist thought around 1870. Stocking (1987: 149) rightly says that 'in Whately and Argyll, the degenerationist assumptions of biblical anthropology surfaced to become, perhaps for the last time in the realm of serious scientific discourse, central issues of debate.' For the last time, but still central.

A second round

The three authors which published a monograph in 1865, each wrote another text about five years later. McLennan's 'The early history of man' (1869), only 33 pages long, offered 'perhaps the best single summary view of sociocultural evolutionism as it emerged in the mid-1860s' (Stocking 1987: 169). Lubbock's *The Origin of*

20 Argyll offered a curious 'argument from implements' to criticize the idea of progress. Even if the use of stone had everywhere preceded that of other metals, it remained clear that 'the same Age which was an Age of Stone in one part of the world was an Age of Metal in another' (183). Curious, because none of the sociocultural evolutionists had argued that the whole world had gone through Thomsen's ages at the same time. On the contrary, this unevenness of development was the crux of the comparative method.

Civilisation and the Primitive Condition of Man (1870) focused no longer on prehistoric archaeology but exclusively on what the subtitle called the ‘mental and social condition of savages’. And Tylor’s two-volume *Primitive Culture* (1871) offered a long defence of socio-evolutionist methodology and the study of survivals. It is well worth seeing how they reacted upon Argyll’s critique, how their thought evolved and what place they allotted to the use of ethnographic analogy.

All of them engaged with the issue of degenerationism. In 1865 McLennan simply took ‘the continuity and uniform character of human progress’ (1865: 7) for granted; four years later he spent half his article questioning ‘whether men were originally savage or civilized’ (1869: 525). Tylor’s work contained an extended discussion on what he called ‘the degradation-theory’ (I, 32-69) and Lubbock’s monograph was one long reaction against it, including two lengthy appendices against Whately and Argyll. The evolutionists all attacked degenerationism by stressing the ubiquity of progressive development. McLennan studied marriage, technology, language and religion in order to show that ‘in each and all of these there has been development’ (1869: 526). These facts ‘which the Duke of Argyll has so lightly put aside in his case against Sir John Lubbock’ led to the conclusion that ‘the degradation hypothesis cannot be seriously considered’ (533). Tylor turned to prehistoric archaeology, ‘the master-key to the investigation of man’s primæval condition’ (1871: I, 58), which showed an indisputable line of cultural progress (I, 62). He concluded that ‘on the whole, progress has far prevailed over relapse’ (I, 32).

Lubbock contended with each of Argyll’s critiques. He entirely demurred with the remark that there was no causal link between technological skills and moral or mental virtues: ‘There is, I think, a very intimate connection between knowledge and civilisation. Knowledge and barbarism cannot coexist—knowledge and civilisation are inseparable’ (1870: 482). He also rebutted the idea that contemporary savages were outcasts driven to the earth’s extremes, because until recently most of the globe had been peopled by primitive races. Rather than being expelled from a prodigious heartland, people migrated only ‘by peaceful, not hostile force; by prosperity, not by misfortune’ (485).²¹ The peripheral ‘Esquimaux’ were not at all wretched compared to the more central natives of Brazil. On top of that, Lubbock questioned whether all humans were capable of corruption and losing their religion: ‘There is, so far as I know, no evidence on record which would justify such an opinion, and, as far as my private experience goes, I at least have met with no such tendency’ (492). He could outline a progressive sequence of religious attitudes and believed it was ‘a fair argument in opposition to the view that savages are degenerate descendants of civilised ancestors’ (495). Why then adopt Argyll’s ‘melancholy conclusion’ (507)?

A new theme

The example of religious development testified to two changes which took place between the first and the second round of social evolutionism: the increased stress on progress and the greater attention for religion. In 1865 Lubbock spent only

21 This view is well in line with his earlier idea that migration was a sign of civilization.

six pages to religion; in 1870 three chapters were devoted to it, claiming around 200 pages of the book's 500. Tylor's *Primitive Culture* (1871) had no less than seven substantial chapters (out of thirteen) dealing with primitive religion and animism. Why did the interest in material culture shift to the topic of spirituality? Replying to Christian-inspired thinkers, the evolutionists showed that religion itself was not the result of divine revelation but a historical development of human civilization. God no longer modelled human progress, but human progress modelled God. Tylor vehemently attacked religious dogmatism in the study of man. According to him, the most extreme partisans of Anglican scholars, and especially archbishop Whately, represented:

[...] a system so hateful to the man of science for its suppression of knowledge, and for that usurpation of intellectual authority by a sacerdotal caste which has at last reached its climax, now that an aged bishop can judge, by infallible inspiration, the results of researches whose evidence and methods are alike beyond his knowledge and his mental grasp. On the other hand, intellect, here trampled under the foot of dogma, takes full revenge elsewhere, even within the domain of religion, in those theological districts where reason takes more and more the command over hereditary belief, like a mayor of the palace superseding a nominal king. (1871: II, 450)

The sudden ethnographic interest for religion was a 'full revenge'. The degenerationists had subjected evolution to religion (Whately by invoking divine intervention, Argyll by invoking human fallibility), now the evolutionists subjected religion to evolution. Ethnography of religion, Tylor argued, appeared 'to countenance the theory of evolution in its highest and widest sense' (II, 452).²² The interest for primitive religion would eventually dominate Victorian sociocultural anthropology during the last quarter of the nineteenth century and beyond.

A new theory

Degenerationism also gave rise to a more rigid definition of societal progress. More than a frequent phenomenon, it was now seen as a nomothetic principle: 'there was a law of progress in the evolution of forms of domestic grouping, which may be enunciated as a law of human progress' (McLennan 1869: 528). A belief in progress was not new, but the idea that it occurred through *stages* was increasingly stressed.²³ Tylor, for instance, said that the 'various grades may be regarded as stages of development of evolution' (1871: I, 1). Progress was also increasingly understood as a global, unilinear process. McLennan could produce 'numerous examples of all the stages [...] occurring among the most diverse races of man' (1869: 527). For Tylor (1871: I, 37) the advance of culture was regarded 'as taking place along one general line'. He boldly stated:

22 Lubbock and Tylor were far from being atheists, they only refused to base scientific inquiry on Christian doctrine. They looked for what Tylor called an 'enlightened Christianity' (1871: I, 23).

23 This gradualist vision of progress obviously echoed the Enlightenment theories of human history.

The educated world of Europe and America practically settles a standard by simply placing its own nations at one end of the social series and savage tribes at the other, arranging the rest of mankind between these limits according as they correspond more closely to savage or to cultured life. (1871: I, 26).

Unilinear progress was so much taken for granted that ‘few would dispute that the following races are arranged rightly in order of culture:—Australian, Tahitian, Aztec, Chinese, Italian’ (Tylor 1871: I, 27).

Lubbock and Tylor, once favouring theories of migration and diffusion, now advocated the doctrine of independent invention. Lubbock held that ‘several races have independently raised themselves’ and that new ideas ‘arise naturally in very distinct nations as they arrive at a similar stage of process’ (1870: 462, 479). Tylor took a similar turn. According to him, the observed similarity between different races of the earth had now to be ascribed ‘to the uniform action of uniform causes’ (1871: I, 1). This shift from historical connection to independent invention was also noted by no one less than Franz Boas (1896: 270): ‘While formerly identities or similarities of culture were considered incontrovertible proof of historical connection, or even of common origin, the new [evolutionist] school declines to consider them as such, but interprets them as results of the uniform working of the human mind.’

Progress was lawlike and unilinear, it occurred through discrete sociocultural stages, and it was reached over the globe by independent invention: this emblematic version of social evolutionism emerged in the context of degenerationist polemics. As a result, the image of the savages themselves changed: though their lifestyle was still seen as ‘very abhorrent’, they nonetheless possessed the germs for autonomous advance so that they ‘do not act without reason, any more than we do’ (Lubbock 1870: v, 21).

A favourite method

This view of progress could not leave the use of ethnographic analogy unaffected. The method of drawing developmental sequences by ethnographic sampling was now valued as the most promising application, especially because the resultant conjectural line could be corroborated with evidence from modern survivals—a point stressed by both Tylor and McLennan.²⁴ Tylor gave the clearest methodological exposé. Since the aim was to

²⁴ If the use of ethnographic analogy was fortified, so was Tylor’s doctrine of survivals. More fiercely than before, *Primitive Culture* defended the value of studying ritualized and symbolical behaviours in modern civilization as relics of what were once actual practices. In an excellent, if somewhat presentist study in ‘the history of scientific method’, Margaret Hodgen argued already in 1937 that Tylor’s more explicit defence of the doctrine of survivals emerged at a time he was ‘in need of an argument to defeat degenerationism’ (1937: 50). If civilization was the result of a slow, gradual progress from a state of savagery, ultimate proof of this lowly origin had to be sought in civilization itself. This is what the study of survivals came down to. Hodgen’s is one of the few studies which stresses the importance of degenerationism in the development of classical evolutionism.

'work out as systematically as possible a scheme of evolution' (1871: I, 20), one had to draw a single line of assumed development and fill it with well-chosen ethnographic and prehistoric instances.²⁵ Tylor said:

By comparing the various stages of civilization among races known to history, with the aid of archaeological inference from the remains of pre-historic tribes, it seems possible to judge in a rough way of an early general condition of man, which from our point of view is to be regarded as a primitive condition. [...] This hypothetical primitive condition corresponds in a considerable degree to that of modern savage tribes [...] in spite of their difference and distance. (1871: I, 21)

Echoing McLennan's creed that old meant 'old in structure, not in chronology', Tylor confidently spoke:

Little respect need be had in such comparisons for date in history or for place on the map; the ancient Swiss lake-dweller may be set beside the mediæval Aztec, and the Ojibwa of North America beside the Zulu of South Africa. (1871: I, 6)

That such ethnographic sampling entailed circular reasoning was apparently not noted (Nisbet 1969: 204; Mandelbaum 1971: 106; Fabian 1983).²⁶

McLennan endorsed this method. His stage-like vision of the past gave rise to a concentric concept of cultural geography. In a major intellectual and industrial centre like London, there were still 'night street-prowlers [...] nearly as low in their habits as the jackals of Calcutta' (542). Beyond the city, the remote areas of the British countryside presented traces of backwardness: 'In Devonshire and Cornwall, at one extreme, and in the Highlands and the Hebrides, at the other, we discover remains of pre-Christian customs and superstitions, as well as modes of life of striking rudeness' (543). This was repeated on a global scale: the uttermost ends of the earth represented the lowest form of primitiveness—not because savages were driven there by superior races but because they 'have been situated where they now are since the dawn of history' (545). Civilization was like a stone thrown in the Thames at London; from this epicentre smaller waves reached the shores of Cornwall and the Hebrides while the last ripples touched the coasts of Tierra del Fuego and Tasmania. Differential stages of progress could thus be found across the globe.

Even Lubbock moderated his quest for a common denominator in favour of drawing evolutionist sequences from simple to complex. 'A comparison of savage tribes belonging to different families of the human race' enabled to arrange evidence in preestablished, conjectural sequences from low to high, from savage to civilized, from early to late, from past to present (1870: 2-3).

25 Tylor's later introduction to anthropology (1881) entirely rested on this simple principle of filling the gaps of preestablished sequences with prehistoric and ethnographic evidence.

26 Nisbet (1969: 204) even said that 'one would not wish to count up the elements of the self-fulfilling, the self-sealing, and the purely circular in this whole mode of analysis.' He called the comparative method 'one of the outstanding examples in all social thought of circular reasoning' (1969: 190).

Three important changes can be noted in comparison with early evolutionism. The first one was that ethnographic analogy dealt no longer with technology. Instead of skinscrapers and fire-making, Lubbock and Tylor now worked on laws, morals, social customs, language, and religion. Prehistoric archaeology was no longer important once the antiquity of man and the reality of progress had been demonstrated; but the method of ethnographic analogy could be transferred to other realms. A second change concerned the gradual decline of piecemeal analogy. Lubbock still warned against projection: ‘It must not be supposed, however, that the condition of man is correctly represented by even the lowest of existing races’ (1870: 2). But Tylor came close to equalling certain cultures with stages of a unilinear sequence:

The Englishman, admitting that he does not climb trees like the wild Australian, nor track game like the savage of the Brazilian forest, nor compete with the ancient Etruscan and the modern Chinese in delicacy of goldsmith's work and ivory carving, nor reach the classic Greek level of oratory and sculpture, may yet claim for himself a general condition above any of these races. (1871: I, 28)

The third change entailed that similarity was even more important as an analogical criterion than before. Argyll had argued that present-day savages and prehistoric people were quite distinct; the evolutionists riposted by emphasizing the resemblances. It was for that reason that Tylor could place the Aztec next to the Swiss lake-dweller. Because ‘existing savages are not the descendants of civilised ancestors’ and because ‘the primitive condition of man was one of utter barbarism’ (Lubbock 1870: 462), it was no more reasonable than to argue that ‘the savage state in some measure represents an early condition of mankind’ (Tylor 1871: I, 32). The analogies formulated after the degenerationist critique thus dealt with broader themes, were inclined to projection, and were strictly based on similarity.

Morgan’s scheme

The epitome of classical evolutionism is without doubt Morgan’s *Ancient Society* (1877). Building upon the three great previous debates, it refined the ‘extremely useful’ three-age system (8), accepted ‘the great antiquity of mankind’ (v), and named degenerationism ‘no longer tenable’ (8). Morgan could do away with it in two sentences:

It came in as a corollary from the Mosaic cosmogony, and was acquiesced in from a supposed necessity which no longer exists. As a theory, it is not only incapable of explaining the existence of savages, but it is without support in the facts of human experience. (1877: 8)

Now, the road lay open free for theoretical systematization. On the basis of an economic criterion, Morgan developed his well-known, sevenfold scheme from savagery (lower, middle, upper), over barbarism (lower, middle, upper) to civilization. It pivoted around the ‘grandly impressive’ notion of ‘a natural as well as necessary sequence of progress’ (553, 3)—process, of course, being understood

as gradualist, unilinear, and independently arrived at. As well as parallel, Morgan added, since technological and economic developments were flanked by social, political and legal improvement. Probably also by religious growth, but this was so ‘grotesque and to some extent unintelligible’ that ‘it may never receive a perfectly satisfactory explanation’ (6, 5). All in all, the classification through the criterion of subsistence replaced the strictly technological yardstick of Lubbock and Tylor. He thus came in close touch with the schemes proposed by Nilsson and the Scottish Enlightenment.

In terms of ethnographic analogy, Morgan sought ‘the best exemplification of each status’ (16) and found that ‘with the exception of the strictly primitive period, the several stages of this progress are tolerably well preserved’ (7). He thus correlated his evolutionist stages with ‘the principal tribes of mankind [...] according to the degree of their relative progress’ (10). Australia and Polynesia were ‘in savagery, pure and simple’, Africa was ‘an ethnical chaos of savagery and barbarism’, while the Indians of America ‘exemplified the condition of mankind in three successive ethnical periods’ (16). Like Wilson and Lafitau, the American Morgan believed that ‘the history and experience of the American Indian tribes represent, more or less nearly, the history and experience of our own remote ancestors when in corresponding conditions’ (vii). He concluded the survey as follows:

Commencing, then, with the Australians and Polynesians, following with the American Indian tribes, and concluding with the Roman and Grecian, who afford the highest exemplification respectively of the six great stages of human progress, the sum of their united experiences may be supposed fairly to represent that of the human family from the Middle Status of savagery to the end of ancient civilization. Consequently, the Aryan nations [i.e. the Indo-Europeans] will find the type of the condition of their remote ancestors, when in savagery, in that of the Australians and Polynesians; when in the Lower Status of barbarism in that of the partially Village Indians of America; and when in the Middle Status in that of the Village Indians, with which their own experience in the Upper Status directly connects. (17)

Morgan’s use of the comparative method was the extreme consequence of what Tylor had embarked upon. He unhesitatingly equated tribes with stages. Instead of using curious habits of fire-making, specific myths, or some enigmatic tools in a piecemeal fashion, now entire tribes were holistically projected onto a unilinear chain of progress. As with McLennan and Tylor, this implied a disregard for chronology: ‘the condition of each [tribe] is the material fact, the time being immaterial’ (13, original emphases).

A zenith of similarity

Around 1870, a version of social evolutionism developed which became known as the textbook version of Victorian anthropology. The comparative method reached its zenith and similarity formed its *raison d'être*. Morgan levelled the differences between present and prehistoric primitives and Tylor was most explicit about this ‘similarity and consistency of phenomena’ which ‘the character and habit of

mankind at once display': 'this similarity and consistency may no doubt be traced, and they may be studied with especial fitness in comparing races near the same grade of civilization' (1871: I, 6). He even went back to Samuel Johnson:

As Dr. Johnson contemptuously said when he had read about Patagonians and South Seas Islanders in Hawkesworth's Voyages, "one set of savages is like another." How true a generalization this really is, any Ethnological Museum may show.
(1871: I, 6)

The tools and customs of savages repeated themselves with 'wonderful uniformity' and even when it came to comparing 'barbarous hordes with civilized nations',

[...] the consideration thrusts itself upon our minds, how far item after item of the life of the lower races passes into analogous proceedings of the higher, in forms not too far changed to be recognized, and sometimes hardly changed at all. (Tylor 1871: I, 6-7)

Indeed, to the evolutionists, 'one set of savages was like another'.²⁷ The ideas on cultural hierarchy and societal progress were combined into a practice of substituting one for another. What once had been a procedure for explaining oil lamps and skinscrapers was now a matter of projecting on the basis of assumed identity.

This trust in similarity predictably resulted in a neglect of the differences between distinct forms of savagery. McLennan wrote: 'Numerous and striking as the differences are by which the types are distinguished, [...] the various races have so much in common that their differences may be disregarded' (1869: 545). Regularity had become more important than patterning variation. In *Pre-Historic Times*, Lubbock still emphasized that 'the differences observable in savage tribes are even more remarkable than the similarities' (1865: 455); but in *The Origin of Civilisation*, he believed that progress followed 'a very similar course even in the

27 A similar climate of opinion prevailed amongst physical anthropologists who sought to establish as much similarities between prehistoric and primitive races. When studying contemporary equivalents for the Neanderthal skull, Huxley (1863: 155) had to force the amount of resemblance: 'A small additional amount of flattening and lengthening, with a corresponding increase of the supraciliary ridge, would convert the Australian brain case into a form identical with that of the aberrant fossil.' And the Upper Palaeolithic skeletons discovered in the last quarter of the century at French sites like Chancelade and Grimaldi were likened to the Eskimo and negroid race respectively. Even if physical anthropology and human palaeontology made little direct impact on the sociocultural evolutionists, we see how in the study of anatomy, like in the study of behaviour, similarity between present and past races was eagerly sought and found. Indeed, the very notion of 'race' was central to all these disciplines and referred to a complex conglomerate of anatomical, cultural and technological signifiers.

It would go too far to discuss the history of the nineteenth-century race concept (though I have embarked upon this theme elsewhere (Van Reybrouck 1998c)). Suffice it to say that for the sociocultural evolutionists it was a common but rarely defined notion. As with all such popular terms (like 'environment' today), its meaning was a semantic cluster, which in this case entailed technological, anatomical, linguistic, ethnic and cultural connotations. It was only in the first decades of the twentieth century that 'race' was reduced by many anthropologists to a strictly biological criterion, though this did not impede the development of a new forms of racial thinking which reached its sad apex in the 1930s and '40s. In the framework of the New Evolutionary Synthesis 'race' started to be studied insofar it could be considered as equivalent to a biological 'population'; in the second half of the twentieth century the concept has been gradually abandoned.

most distinct races of man' (1870: 3). Tylor, too, found 'such regularity in the composition of societies of men, that we can drop individual differences out of sight':

[...] just as, when looking down upon an army from a hill, we forget the individual soldier, whom, in fact, we can scarce distinguish in the mass, while we see each regiment as an organized body, spreading or concentrating, moving in advance or in retreat. (1871: I, 11)

Similarity was stressed at the expense of equally obvious difference as part of the anti-degenerationist reaction. If similarity had been the taken-for-granted bottom-line of previous ethnographic analogies, now it was the explicitly acknowledged cornerstone of the projections from present source to past target. As always, it was Morgan who gave the tenet its most candid defence:

So essentially identical are the arts institutions and mode of life in the same status upon all the continents, that the archaic form of the principal domestic institutions of the Greeks and Romans must even now be sought in the corresponding institutions of the American aborigines. (1877: 17)

Contemporary savages were, in fact, contemporary ancestors.

Evolutionist fragmentation

In the last two decades of the nineteenth century, sociocultural evolutionism increasingly turned away from the study of material culture and focused on the development of social institutions and religious ideas. While Tylor increasingly sought to correlate the lowest stage of savagery with the Tasmanians, such a comparative reasoning was shunned by other evolutionists like Spencer and Frazer and was criticized by Westermarck and Boas. Prehistoric archaeology, once the 'master-key' for proving sociocultural progress, was now no longer needed. Anthropology and archaeology grew increasingly independent from each other.

Archaeology and anthropology diverge

Anthropology's growing independence is undeniable in the end of the century's two greatest evolutionist achievements: Herbert Spencer's *Principles of Sociology* (1876-1896) and James Frazer's *The Golden Bough* (1890). Their cyclopic proportions notwithstanding (Spencer's book appeared in three tomes, Frazer's original two volumes were expanded to twelve by 1914), both books differed profoundly in theme. The *Principles* offered a grandiose panopticon of sociocultural growth in political, ecclesiastical and industrial institutions; *The Golden Bough* zoomed in on the ritual killing of the priest in primitive religion. Nonetheless, the role of prehistory in both was minimal. As a preparation for the redaction of his *Principles of Sociology*, Spencer (and his assistants) systematically assembled masses of ethnographic evidence into a series of eight folios entitled *Descriptive Sociology*. Numerous societies were classified into cyclopean tables and indexes, but pre-

historic materials were conspicuously absent. There were no parallels between Europe's prehistoric past and the 'types of Lowest Races, Negrito Races, and Malayo-Polynesian races' (Spencer 1874). Spencer only fell back upon the comparative method 'to classify the types of structure and to establish their sequences' (Andreski 1969: xviii). Rather than prehistoric reconstruction, his work was an attempt at 'evolutionary typology' (Andreski 1969: xviii; cf. Carneiro 1967). Frazer, on the other hand, was directly indebted to Tylor's ethnography of religion. Accepting that modern religion sprung from primitive antecedents, Frazer set out to undertake a study in 'comparative religion'. His sources were twofold: as a classicist he heavily relied on classical mythology; and as a student of Tylor, he greatly valued survivals: 'Popular superstitions and customs of the peasantry,' he wrote, 'are by far the fullest and most trustworthy evidence we possess as to the primitive religion of the Aryans' (1890: viii). His comparative method did not proceed by ethnographic parallels or wholesale projections from the savage state. Subsequent expansions of *The Golden Bough* incorporated a myriad of additional evidence, but these were strictly ethnographic.

Along with this evolutionist anthropology devoid of archaeology, there developed an evolutionist archaeology devoid of ethnography. This is well exemplified by the intellectual career of the leading authority on British prehistory after Lubbock: General Pitt Rivers (Chapman 1985; Bowden 1991). During the 1860s and 70s he endorsed the merits of the comparative method (Pitt Rivers 1874), but after 1880 his work became more strictly archaeological. In his early excavations in Ireland he did not hesitate to draw a parallel with contemporary Eskimos.²⁸ But having inherited the enormous estate at Cranborne Chase in Wiltshire, he spent several years excavating barrows, dykes and ditches, but avoided ethnographic reasoning throughout. This does not mean that he gave up his evolutionist principles, only that the comparative method held no longer sway. The method even lost ground in Palaeolithic research. James Geikie found an equation between Magdalenians and Eskimos 'a simple assumption': even if some formal similarities can be found, 'the coincidence is not startling' (Geikie 1881: 548, 549). In France, increasingly the centre of Palaeolithic research, there were several attempts at refining the chronology by scholars like Lartet and De Mortillet. The latter's enormously influential framework presented a rigid unilinear view of prehistory, but it was not filled with contemporary ethnographic data (Richard 1992: 25-7). He described '*le grand développement [...] de l'humanité à l'état sauvage*' (De Mortillet 1883: 22) without recourse to extant savages. '*Paléoethnologie*', as he preferred to call prehistoric archaeology, was despite its term devoid of ethnology.

28 Ethnography and prehistory were intimately intertwined in his thinking at this stage of his life. He purchased great amounts of ethnographic artefacts from travellers and organized these according to the principle of continuity of typical form. Just like Tylor arranged the habits of fire-making and forms of religion along a developmental line, Pitt Rivers ordered boomerangs, spears, shields and the like into continuous chains of gradual perfection. His principle was that 'every form marks its own place in sequence by its relative complexity or affinity to other allied forms' (1874: 12). This clearly comparative approach also surfaced in the museological guidelines he formulated for ethnographic displays (Chapman 1985). Archaeology essentially served to lengthen and complete the typological sequences he had already established on the basis of ethnography (Bowden 1991: 55).

In prehistoric archaeology, the last two decades of the nineteenth century were increasingly devoted to refining the chronological framework. Montelius' division of later European prehistory made clear that there was not one line of development but an enormous diversity in time and space. Ethnographic comparison between stages became therefore quite problematic. Boas noted that thanks to such detailed archaeological studies on 'the multiplicity of converging and diverging lines', the grand system of the evolution of culture was 'losing much of its plausibility' (1904: 271). The discovery of Palaeolithic rock art—first attested in Altamira in 1878, later confirmed by the caves of Font de Gaume and La Mouthe (Groenen 1994: 317-25)—also contributed to the decline of the comparative method. So unlike anything known from ethnography, it undermined the long-held assumption of a fundamental similarity between prehistoric and present savages. In his famous 'mea culpa of a sceptic', the French prehistorian Emile Cartailhac conceded for the authenticity of the cave art and wrote: '*En vain on fait appel à l'imagination, en vain à toute ethnographie! Les renseignements ont beau venir de loin, même du Transvaal, de l'Australie, du Nord-Amérique, rien ne peut permettre de soupçonner pourquoi ces surfaces étaient ornées ainsi?*' (Cartailhac 1902).²⁹

Tylor and the Tasmanians

Several archaeologists and anthropologists began to shun comparative reasoning, but Tylor finally found what he had long been looking for: a living Palaeolithic tribe, or at least an only recently exterminated one. In 1890, H. Ling Roth published *The Aborigines of Tasmania*, a compilation of all available evidence on the Tasmanians from the form of the skull to the method of making fire. The preface was written by Tylor who called their extinction 'a dismal page of our colonial history' (1890: vii) but was relieved by the 'absolute completeness' (v) of the volume. His old despair about the unlikelihood 'that any tribe known to modern observers should be anything like a fair representative of primary conditions' (1865: 369) was now optimistically refuted with 'the vestiges of a people so representative of the rudest type of man' (1890: vii):

If there have remained anywhere up to modern times men whose condition has changed little since the early Stone Age, the Tasmanians seem to have been such a people. They stand before us as a branch of the Negroid race illustrating the condition of man near his lowest known level of culture. (Tylor 1890: v)

In a host of publications between 1890 and 1900, at a time when the comparative method came under fire, Tylor avidly expanded this Tasmanian projection (Tylor 1890; 1894; 1895; 1899a; 1899b; 1900).

At first Tylor was struck by the similarity between Tasmanian and Palaeolithic tools (figure 7). A specimen sent to him from the antipodes (Tylor 1895; cf. Murray 1992) affirmed that they were 'corresponding exactly' (1890: v) so that the analogy was justified 'by the workmanship of their stone implements' (v). Tylor's insistence on tools in the preface of a book which paid little attention to lithic technology, refers to his long-

29 New studies, like the ones by Spencer and Gillen on the Australian Arunta (1899) and Stow on Bushmen cave art (1905), would soon receive prehistoric interest from authors like Breuil and Sollas (cf. infra).

standing conviction that technical mastery was a barometer of overall progress. Tools were ‘pointers in the study of civilization’ (1871: I, 62). In a subsequent paper, he therefore embarked upon ‘the study of their culture in other respects’ (Tylor 1894: 149). Arts, language, religion, morals, social institutions, ‘just as their stone implements belong to the recognized stone age, though at an especially low level’ (149). Tasmanians were not degenerated but had been in a condition of ‘normal or healthy savagery’ (149). Tylor’s culture concept was holistic: since all parts of a society belonged to the same stage, knowing the degree of progress in one realm (technology) was enough to extrapolate it to other realms. In a given culture, he had once written, ‘evidence as to the condition of any one of its departments really does authorise, in some measure, an opinion as to its condition as a whole’ (1869: 11). On the basis of a side-scraper, Tasmanians could literally become ‘living representatives of the early Stone Age’ (148–9). He eventually went so far as to invoke anatomical evidence on ‘the similarities between the modern Australoid [sic] skulls and the prehistoric skulls of Neanderthal, Spy, Padbaba, etc.’ (1899a: 199). Tylor thus saw his initial hypothesis further confirmed. Archaeology, ethnography and physical anthropology converged to the point that ‘Man of the Lower Stone Age ceases to be a creature of philosophic inference, but becomes a known reality’ (1899b: ix).

But not everyone was convinced. Henry Balfour, curator of the Pitt Rivers museum in Oxford, argued that Tylor risked to neglect relevant dissimilarity between the source and the target: ‘It is essential that the great difference between the environmental conditions—climate, geographical surroundings, etc.—under which the two races lived, should not be overlooked’ (in Tylor 1900: 260). The observed similarity on which the analogy rested was also questionable. ‘The most characteristic tool of the Tasmanians’, i.e. the simple retouched scraper, was by no means typically Palaeolithic but proved to be ‘the most persistent of all stone implements’ (260). Knowledge about Tasmanians was after all ‘based largely upon *post-mortem* study’ (261). Balfour doubted the relevance of Tasmanian tools, but others went much further and a new anthropology was emerging by the end of the nineteenth century.

Critique of evolutionism

‘How can we from ethnographical facts acquire information regarding the early history of mankind?’ The question was posed by Edward Westermarck in the introduction of his influential *History of Human Marriage* (1891: 3). Westermarck, originally working as a lecturer in sociology in Helsinki before he became a professor at the London School of Economics, addressed the problem McLennan had dealt with: the origin of marriage. Despite ‘the admirable works of Dr. Tylor, Sir John Lubbock, and Mr. Herbert Spencer’, he felt ‘that the scientific value of the conclusions drawn from ethnographical facts has not always been adequate to the labour, thought, and acumen bestowed on them’ (2). He agreed with McLennan that savage races and symbolic survivals were the main sources to reconstruct early human history but the wide-ranging difference of opinion was ‘due, not to the material, but to the manner of treating it’:

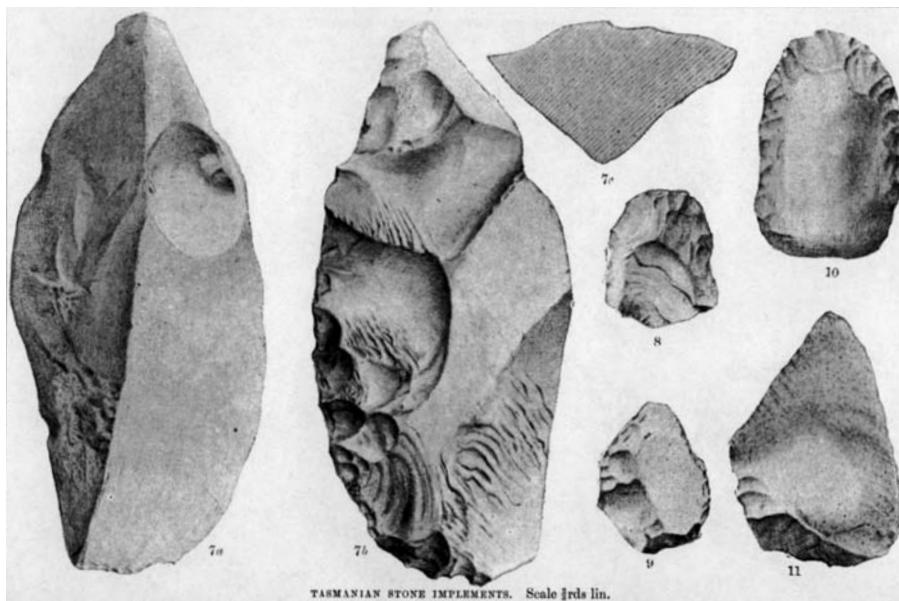


Figure 7. Resemblances in stone tools led Tylor to equate the entire Tasmanian culture with that of the lowest stage of savagery during the Upper Palaeolithic (Tylor 1894: plate XI)

Nothing has been more fatal to the Science of Society than the habit of inferring, without sufficient reasons, from the prevalence of a custom or institution among savage peoples, that this custom, this institution is a relic of a stage of development that the whole human race once went through. (2)

Westermarck laid bare the circular reasoning which many social evolutionists had been guilty of. One had to ‘avoid assuming a custom to be primitive, only because, at the first glance, it appears to be so’ (6).

Was there any alternative that kept the student of society ‘on his guard against rash conclusions’ (6)? Westermarck believed there was. Rather than projecting from source to target, attention should be given to the causal patterns within the source. Westermarck: ‘We have first to find out the causes of the social phenomena; then from the prevalence of the causes, we may infer the prevalence of the phenomena themselves’ (4). He refused to favour single tribes as representative, but studied the whole range of contemporary savagery and included some evidence on primate behaviour. Finding causes in the present source was not enough if we were still ‘quite ignorant whether the causes in question operated or not in the past’ (5). By invoking Lubbock’s notion of universal progress and Darwin’s theory of a human-animal continuum, Westermarck showed that a uniformitarian assumption was legitimate. Westermarck thus amended traditional evolutionist methodology by improving the structure of its analogy: immediate similarity was no longer enough; the causes operating in the source had to be understood; the source side of the analogy was expanded; and ground for a uniformitarian assumption was established. However, the substantial part of his work was still heavily dependent on the previous evolutionist generation. His narrative of marriage forms among animals and humans

belonged to the sort of unilinear sequence which Tylor had proposed, the only difference being that animals were added to it.³⁰ His work is nonetheless indicative of the doubts that emerged within the evolutionist paradigm.

Franz Boas was much more sceptical. His intellectual background, like Westermarck's, laid outside British anthropology: it was fed by German ethnology, geography, physics, and physical anthropology (Stocking 1974b: 12; cf. Kuper 1988: 125-30; Boas 1938). Having seen already the comparative method's excesses (in the work of Bastian) and opposition (in the work of Virchow, Boas' teacher), Boas developed a profound critique against it after his move to the Anglo-Saxon scene. Already in one of his earliest paper, he greatly differed from the evolutionist paradigm (Boas 1888). Looking at the schemes of progress from savagery to civilization, he simply believed that 'the cautious scientist cannot follow those vagaries' (637). He sought to introduce to ethnology the historical mode of inquiry which had been abandoned by Tylor, McLennan, Lubbock and Morgan. Time did again matter since 'the life of a people in all its aspects is a result of its history' (632); historical connection became again respectable to explain cross-cultural similarity; and degeneration was believed to be genuine. His style of arguing was also at odds with the evolutionist orthodoxy. More abstract than the high prose of Victorian scholars, his arguments relied less on the enumeration of exemplary cases but proceeded by analytical precision and terse writing.³¹

In 1896 the journal *Science* published Boas' article 'The limitations of the comparative method of anthropology'. This landmark in the history of anthropology entailed an elaborate critique of evolutionism which saw proof of its self-proclaimed unilinear progress in the world-wide similarity of cultural customs, ideas, and practices.³² Boas found the 'grand system of the evolution of society as of very doubtful value' (276). Firstly, because it was based on 'observed homologies and supposed similarities' (1904: 263). Believing that 'we are no longer prone to infer from superficial similarities' (1888: 636), he wanted that 'before extended comparisons are made, the comparability of the material must be proved' (1896: 275). Secondly, he criticized the evolutionists' selective empirical sampling. In order to uphold their basic principle, users of the comparative

30 Westermarck's progressionist model also echoed the ideas set forth by Darwin in the *Descent of Man* (1871). In contrast to his earlier work, Darwin adopted a more linear than branching model when he was dealing with the evolution of humanity. His own position thus became 'fundamentally at odds with the argument set out in the *Origin of species*' and resulted in a fairly non-Darwinist strand (Ingold 1986: 30, 47-50; cf. Bowler 1988: 143-5; 1990: 192). It was also only in the *Descent* that Darwin first staged the Fuegians as relics of the remote past, an idea which never surfaced in the account of his travels aboard the Beagle forty years earlier (Bowler 1992). Westermarck's sequence of parenthood from invertebrate animals to civilized humans followed this linear form of reasoning.

31 Another aspect where young Boas differed fundamentally from the social evolutionists was in his ideas on museum layout. Unlike Thomsen, Worsaae, Wilson and Pitt-Rivers who had all defended a typological and developmentalist system, Boas, again indebted to a German tradition, sought to arrange ethnographic materials in geographical and tribal terms (Bunzel 1962: 5).

32 Though Boas directed his critique at evolutionists in general, the German anthropologist A. Bastian was the direct target of his paper. Still more than Tylor or Morgan, Bastian had dogmatically defended world-wide unilinear evolution by independent invention on the basis of cross-cultural similarities. Bastian's extremism had a certain autodestructive influence on the further history of evolutionism, just like Sollas' extremist reasonings would cast a dark shadow over the comparative method.

method were often ‘forcing phenomena into the straitjacket of a theory’ (277). Thirdly, he denounced the uniformity principle of the evolutionist paradigm:

Anthropological research which compares similar cultural phenomena from various parts of the world, in order to discover the uniform history of their development, makes the assumption that the same ethnological phenomenon has everywhere developed in the same manner. Here lies the flaw in the argument [...] for no such proof can be given. Even the most cursory review shows that the same phenomena may develop in a multitude of ways. (273)

How was one to remedy all those deficiencies? Like Westermarck, Boas urged to study the causes underlying cultural phenomena. To understand a phenomenon ‘we must demand that the causes from which it developed be investigated and that comparisons be restricted to those phenomena which have been proved to be effects of the same causes’ (1896: 275). Similarity, i.e. the horizontal relation of the analogy, was not enough unless the vertical relation of causality was taken into consideration. This also entailed an awareness of the problem of equifinality, namely that ‘other causes could possibly lead to the same ideas’ (276; cf. Stocking 1974b: 2). Such an understanding could only be reached by ‘the much ridiculed historical method’ (277). To Boas, the historical method was far superior to the comparative method and much closer to the Darwinian principle of branching, anti-teleological evolution than sociocultural evolutionism (Boas 1888: 633; Ingold 1986: 30, 65; Bowler 1988: 140).

His anti-evolutionist emphasis notwithstanding, Boas still withheld the old ambition of finding general laws. Historical particularism was, originally at least, a methodological device for a larger nomothetic project.³³

If anthropology desires to establish the laws governing the growth of culture, it must not confine itself to comparing the results of the growth alone, but whenever such is feasible it must compare the processes of growth, and these can be discovered by means of studies of the cultures of small geographical areas. (280)

Rather than glossing together evidence from a world-wide survey, one had to focus on ‘a well-defined, small geographical territory’ (1896: 277). The time of the armchair anthropologist was gone, and fieldwork increasingly became a part of the scientific enterprise. The moment Boas’ paper was published, Baldwin Spencer and Frank Gillen, motivated by Tylor and Frazer, were undertaking their seminal fieldwork on the Arunta rituals in Central Australia. Boas himself would soon start his fieldwork with Kwakiutl and Nootka of the American North-West coast. Eventually, this type of ‘careful and slow detailed study of local phenomena’ (1896: 277) contributed to a decline in diachronic and nomothetic interests (Harris 1968: 170; Gosden 1999). Insisting on understanding individual societies prevented a return to the question on the evolution of culture. Tylor, Lubbock,

33 It was only much later in his career that Boas let go this law-building ambition in favour of a strictly particularist project (Boas 1920; 1924; cf. Stocking 1974b).

McLennan and Morgan nearly exclusively focused on similarity, but Boas and his students increasingly limited themselves to a study of causality in most particular settings.

The comparative method's swan-song: Sollas

The comparative method lost ground both in archaeology and anthropology, but there were still occasions where it could thrive, even more effervescently than before. W.J. Sollas' *Ancient Hunters* (1911), the first general survey in English on Palaeolithic archaeology since Lubbock, was one of these. The book assembled 'the vast store of facts' (1911: vii) which had been discovered in geology, archaeology, sociocultural anthropology and physical anthropology.³⁴ Sollas followed the classification of De Mortillet and it is well known how he flanked the archaeological chapters on the Acheulean, the Mousterian, the Aurignacian and the Magdalenian with four ethnographic chapters on the Tasmanians, the Australians, the Bushmen and the Eskimos respectively. The study of contemporary primitive societies was not 'a wilful anachronism' because it afforded 'an opportunity of interpreting the past by the present—a saving procedure in a subject where fantasy is only too likely to play a leading part' (70). In up to forty pages per tribe, he gave extended descriptions of the material culture, anatomical peculiarities and, if evidence permitted, social institutions and ritual practices. Just like Morgan illustrated the stages of his progressionist scheme with an example of a modern tribal society, Sollas filled the sterile epochs of De Mortillet's classification with ethnographic cases.

The criterion of similarity provided the underlying rationale for selecting and ordering parallels. The Tasmanians were adopted because of the 'curious exactness' already noted by Tylor between their tools and the ones found in the Dordogne (88). The Australians were defended as 'the Mousterians of the Antipodes' (170) because of their cranial similarity to Neanderthals—a point which Huxley had made fifty years earlier. The Bushmen were selected on the basis of their rock art which recalled 'in the closest manner the best efforts of Aurignacian times' (252). And the Eskimos were privileged because 'the evidence could scarcely be more definite' (376) regarding their anatomical and cultural likenesses with the Magdalenian hunters. The Chancelade skeleton was believed to represent 'a veritable Eskimo, who lived in southern France during the Magdalenian age' (376) and the material culture contained was in perfect accordance:

There is no essential difference between the more primitive Eskimo arrow-straighteners and those of the Magdalenians; the bone arrow-heads are often strikingly similar, and this similarity extends to those used by the Indians [...]; the bone hairpins of the Magdalenians may be matched among those of the Eskimo, and the lobate ivory pendants, sometimes heart-shaped, which both races possess, are almost identical in size and form. [...] Other little pendants of unknown use

³⁴ The time was ripe for syntheses. In the same year, Arthur Keith published *Ancient Types of Man* but this was mostly confined to fossil evidence. A few years later Henry Osborn wrote another synthesis *Men of the Old Stone Age* (Trinkaus and Shipman 1993: 210).

among the Eskimo resemble the Magdalenian in every respect, and this is a very important fact. It is resemblance in trivial detail which impresses us quite as much, if not more, than resemblance in general design. (368, emphases added)

Even if such vital Eskimo inventions like the kayak and the sledge were absent from the Magdalenian repertoire, Sollas guessed that such relevant dissimilarity did not weaken his argument. Superficial similarity, ‘resemblance in trivial detail’, was considered more definite.³⁵

If this read like an archaeological translation of Morgan’s scheme, it was also profoundly different. Progressive change was no longer the result of independent, unilinear development: ‘I find little evidence of indigenous evolution,’ Sollas wrote, ‘but much that suggests the influence of migrating races; if this a heresy it is at least respectable and is now rapidly gaining adherents’ (vii). Thanks to Boas in anthropology and Montelius in archaeology, at the turn of the century diffusion and migration re-emerged as explanatory mechanisms. The Tasmanians were ‘the surviving descendants of a primitive race’ (85) which had once been widely distributed over the old world. The natives of Australia could be seen as ‘the inferior tribes of the Neandertal race [which] were driven by stress of competition out of Europe, and wandered till they reached the Australian region’ (208). The Bushmen, deriving from the Aurignacian parent stock, ‘must have traversed the whole length of Africa before arriving at the Cape’ (301), leaving behind a trace of rock art manifestations ‘between the Dordogne and the Cape’ (304). And the Eskimos were Magdalenians who had been pushed into the north at the end of the ice age when subarctic fauna retreated to Siberia and early agriculture exerted demographic pressure. Sollas could thus sum up his conclusion:

If the views we have expressed in this and preceding chapters are well founded, it would appear that the surviving races which represent the vanished Palaeolithic hunters have succeeded one another over Europe in the order of their intelligence: each has yielded in turn to a more highly developed and more highly gifted form of man. From what is now the focus of civilisation they have one by one been expelled and driven to the uttermost parts of the earth: the Mousterians survive in the remotely related Australians at the Antipodes, the Solutrians are represented by the Bushmen of the southern extremity, the Magdalenians by the Eskimo on the frozen margin of the North American continent and as well, perhaps, by the Red Indians. (382-3)

Sollas is often seen as the epitome of sociocultural evolutionism (cf. Stiles 1977: 89; Wylie 1985: 66), but his diffusionist thinking makes him at the same time the precursor of the direct-historical method for drawing analogies (Trigger 1989: 155; Bowler 1992). His legacy, as we will see in the next chapter, was therefore ambiguous. Archaeologists during the first half of the twentieth century rapidly

35 The Solutrian epoch with its impressive flint implements was not illustrated by a present-day society because ‘no existing tribe is able to obtain quite the same perfection of retouch’ (309). Since no simile existed, no analogy was drawn either; even in the negation of analogy, similarity was the sole criterion at stake.

rejected the excesses of his comparative method—his far-fetched conclusions even cast a dark shadow on the whole of sociocultural evolutionism to the extent that even today archaeologists (Hodder 1982a) consider Victorian anthropology as synonymous to Sollas—yet at the same time, he was still appreciated for his reasoning from historically-connected societies, which was increasingly seen as the only reliable form of analogy.

Divergence of opinion

After Morgan's grand scheme of universal progress, a fragmentation occurred in the study of sociocultural evolution. Archaeology and anthropology became institutionalized (as reflected by the increase of excavations and fieldwork, the emergence of specific journals and museums, the growing number of practitioners, and the expanding scale of scholarly gatherings) and the initially unified discipline made room for a more diversified spectrum of opinion.

The attitude towards ethnographic analogy can be clustered in three distinct groups: a first group was characterized by a decreasing interest for the comparative method; a second group continued to trust the method and benefited from the data disclosed by new publications; a third group explicitly criticized the method. Frazer and Spencer belonged to the first group. Still versed in the evolutionist project, they both avoided ethnographic parallels in the style of Morgan. In archaeology, the work of Pitt Rivers and de Mortillet belonged to this group. Both were firmly committed to a strict unilinear evolutionism, but refrained from drawing ethnographic analogies.

Scholars belonging to the second group perpetuated the use of the Morgan-like comparative method and incorporated new evidence in the existing framework. This is clearly the case for Tylor who found in Ling Roth's compilation on the Tasmanians a confirmation of his earlier views. Sollas walked into Tylor's footsteps in his enthusiasm for the Tasmanians. Further drawing upon Spencer and Gillen's fieldwork in Australia, Stow's in Africa and Boas' in North America, he elaborated the Palaeolithic equation with a native tribe for every age.

Westermarck and Boas represented the third group. They were at pains to point out the deficiencies and fallacies of the comparative method and suggested an alternative procedure. Interestingly, both had come to Anglo-Saxon evolutionist anthropology from a continental background in ethnography—Westermarck from Finland; Boas from Germany. The popularity which the comparative method enjoyed in London anthropological circles had never been so large on the continent. Despite intellectual differences, both lamented the undue reliance on similarity. Reasoning from similarity, without weighing its relevance, was bound to fail. Both, too, suggested that internal causality had to be studied if the ethnographic information was to have any clear value.

At the turn of the century, there was an interesting discrepancy between adherents of the second and third group. Whereas the long-favoured criterion of similarity was brought to unseen heights of quasi-identity in the arguments of Tylor and Sollas, people like Westermarck and Boas suggested that this was not the way to proceed, unless attention be given to causality. The clarity of Sollas'

argument showed at the same time its simplicity; by optimistically revitalizing the comparative method to a schematic extreme, some of the weaknesses had become apparent. In the first half of the twentieth century, Boasian particularism would dovetail nicely with the culture-historical paradigm that arose in prehistoric archaeology.

Conclusion

From Thomsen's typology to Sollas' synthesis, the use of ethnographic analogy played a fundamental role in the nineteenth-century representation of human prehistory. Indeed, 'prehistory has never existed without ethnography' (Orme 1973: 490) and many of its basic insights were suggested or extrapolated from contemporary source contexts. Before moving on to see how this legacy was received in the twentieth-century, it deserves to pay a closer attention to the six strength criteria of the analogical arguments.

Firstly, the *number of source contexts* evidently increased throughout the nineteenth century. Whereas Wilson and Nilsson had to contend themselves with general reports by travellers and missionaries on North-American Indians and South Sea Islanders, someone like Sollas could dispose of detailed ethnographies on the Bushmen, the Central Australians, the Eskimos, and the Tasmanians. In general, nineteenth-century scholars tried to incorporate as much contemporary evidence as possible which in principle would have enhanced the quality of the analogy. However, when it came to drawing specific analogies, the sources were mostly narrowed to the best fitting instance. Thus, Wilson explained his Scottish oil lamps just with the specimens he had found on the Faroe Islands; Tylor associated the 'stick-and-groove' stage of pyrotechnology exclusively with the South Sea Islands; Morgan correlated each stage of his scheme with only one specific society; Sollas strictly found a single contemporary race as representative of each Palaeolithic epoch. Despite the broad ethnographical interests, at the end of the day it came down to selecting the single entity from the source which correlated best with the target entity under consideration. Lubbock's search for a common denominator was the only attempt to go beyond inferences from a single source entity, but he did not pursue it further.

With regard to the related criterion of the *variety of source contexts*, Lubbock, too, was the one who scored best: his image of the prehistoric savage was based on such varied sources as the 'wretched' Tasmanians and the 'nearly civilized' Tahitians. The others, however, by reducing the source analogue to a single well chosen instance, minimized the variety of source contexts—a point criticized by Westermarck's alternative. Source variety was further limited by an exclusive attention to human analogues; inspiration from nonhuman primates was virtually absent (a point which I elaborate in the introduction of Chapter 4; cf. Stocking 1987: 176–7). Lubbock's *Pre-Historic Times* had one line on nonhuman primates, but two hundred pages on modern savages.³⁶ Evidence on primate behaviour was of course scant, in contrast to the number of ethnographic publi-

36 'So long, indeed, as he was confined to the tropics, he may have found a succession of fruits, and have lived as the monkeys do now' (Lubbock 1865: 475–6).

cations. The study of animal behaviour in the nineteenth century was confined to more readily observable species like ants, bees, and beavers. Social evolutionists had considerable interests for these: Morgan wrote on the American beaver when turning to anthropology and Lubbock investigated bees and wasps after his work on prehistory (Ingold 1988; Swetlitz 1988; Clark 1998). On top of that, for most sociocultural evolutionists the question of man's simian descent was simply not yet at stake. They were more interested in human civilization than in human evolution. Most evolutionists 'assumed that even in the most primitive social environment, a human being was still a human being' (Bowler 1989: 39).

Thirdly, how was the situation for the criterion of *similarity*? The question can best be answered by the words of Franz Boas (1904: 267): 'The evolutionary method was based essentially on the observation of sameness of cultural traits the world over.' Indeed, it is no exaggeration to state that throughout the nineteenth century the amount of similarity was the only analogical criterion seriously dealt with. Harris (1968: 171) rightly said that 'it was upon the similarities that a science of universal history [had] depended.' Nilsson and Wilson held that prehistoric and present primitives were essentially similar because they both lived in the Stone Age. Lubbock, Tylor and McLennan reasoned from ethnographic present to prehistoric past on account of 'such similarity, so many correspondences, so much sameness' (McLennan 1865: 3). Geographically distant was interchangeable with geologically remote. They buttressed this similarity even further after some opponents had suggested that today's savages did not resemble the past inhabitants of Europe; similarity thus became the cornerstone of a fully-developed sociocultural evolutionism. Morgan, Tylor and Sollas even believed that entire modern societies were identical to certain stages of the past, the emergent anti-evolutionist criticisms notwithstanding. Despite the changing polemical contexts, formal similarity remained the most important logical foundation of ethnographic reasoning and its centrality even increased as the century moved on. Goodness of fit determined the quality of analogy. Nevertheless, the meaning of this similarity was subject to historical change. For Nilsson and Wilson, the evidence of cross-cultural similarity testified to divine action. For Lubbock and Tylor similarity first revealed the extent of historical connection, and later the prevalence of independent invention. With Sollas, finally, we fall back in an earlier mode of explanation which interpreted similarity in terms of migrations.

Fourthly, *dissimilarity* was a notion which evolutionists felt uncomfortable with. Believed to be structure-violating, every hint of difference was to be avoided. McLennan repeatedly said that because the resemblances between savages were so numerous, the differences could be done away with; Tylor wholeheartedly agreed with that; and Morgan projected present societies on past ones as if there was no difference between the two. The denial of difference went hand in hand with their monogenist convictions, i.e. the belief that all races of the earth represented a single species. Sollas, on the other hand, recognized discrepancies between the source and target of his analogies (e.g. when the Middle Palaeolithic Australians turned out to use ground implements of Neolithic type), but systematically minimized their importance. Neglecting and trivializing, these were two ways to eliminate

dissimilarity. Another favourite procedure consisted in bracketing certain parameters, in particular time. McLennan, Tylor and Morgan all said that time was not material, which enabled them to browse through the centuries and the continents in order to force similarity where it was not found. Boas was the first to direct the attention again to an awareness of time when he said societies could only be understood in terms of their own history. Similarity made place for particularism and drawing analogies became a much more complex affair.

Fifthly, little consideration was given to the notion of *relevance*. In the absence of a definite yardstick that could tell which aspects of the source and target were relevant and which were not, enumerating the points of similarity became the only remaining procedure. Notions like the psychic unity of humanity, the like workings of the mind under like conditions, the mechanism of independent invention (or divine revelation in Wilson's view), and the monogenist conviction all supported the view that similarities between cultures were due to similar causes, but a genuine causal understanding, like Westermarck and Boas called for, of why certain patterns emerged in certain places and why others did not, was never reached by the sociocultural evolutionists. Tylor could argue that technology was correlated to other sociocultural realms, but similarity remained formal similarity: if two entities from the source and the target looked alike in certain respects (regardless of whether it was a stone tool, a ritual or a kinship form), they were alike in other respects (function, meaning, origin). An explanation for this similarity was rarely sought so that evolutionist analogies were formal analogies, hardly ever relational analogies.

Sixthly, how was the *weight* of the analogical conclusions in relation to the weight of the premises? Here, a clear change can be noted. In an earlier phase of the comparative method, the scale was still in balance. Wilson, for instance, could safely infer the function of his oil lamp with its Faroe analogue; Lubbock could identify the use of Palaeolithic skinscrapers on account of Inuit specimens; Evans drew many small-scale parallels between prehistoric and ethnographic objects (1872; 1881). In the 1860s, both Lubbock and Tylor agreed that no contemporary tribe of savages could be seen as directly representing an ancestral condition; they thus preferred piecemeal analogies between well chosen cases. This, however, changed when Morgan started to project entire societies on the steps of his evolutionist ladder. Tylor and Sollas followed in his footsteps. Clearly, the scales turned out of balance now that complete stages of the prehistoric past were reconstructed on the small base of observed similarity. Tylor's Tasmanian analogy, in its initial form, rested on the very thin line of formal resemblance of *one* Tasmanian scraper to its Palaeolithic equivalent (Murray 1992). Sollas' equations had at their base often nothing more than a vague resemblance in rock art or skull form's. Geikie's warning that 'one specimen only seems too slight a foundation to build such a theory' (1881: 549) was passed in silence; their conclusions clearly outweighed the premises. Just like in earlier centuries, the role of ethnographic analogies in the nineteenth century oscillated between localized problem-solving with high degrees of inferential confidence and global visualization with lesser degrees of such confidence.

Ethnoarchaeology

The dormancy of ethnographic analogy

In 1911, the same year in which W.J. Sollas published his *Ancient Hunters*, another book appeared which, despite its mere thirty pages, would change the face of prehistoric archaeology. It was Gustaf Kossinna's *Die Herkunft der Germanen*, a treaty on the method of prehistoric research illustrated by means of a contentious example. Though the author did not belong to Anglo-Saxon archaeology, his influence on it was substantial, particularly through the work of Childe. The late-nineteenth-century increase in archaeological excavations had shown that there were vast variations in the archaeological record from different regions. Kossinna took this geographical variability as the cardinal point of his thought: the task of the prehistorian was to delineate cultural provinces (*Kulturkreisen*) and to detect the amount of contact between them. His axiom was: '*Kulturgebiete sind Volksgebiete*' (1911: 4), regional variation in material culture had to be understood in ethnic terms. His was an approach which thus identified '*Töpfer mit Völkern*' (11), pots with people, and which used similarities and differences between cultural provinces as arguments for migration of peoples, diffusion of ideas, or simple contact. Likewise, the year 1911 witnessed the publication of another important, if less spectacular, monograph: John Myres' *The Dawn of History*. Myres, once the editor of a Pitt Rivers reader (1906), left the latter's evolutionist thought far behind in his own substantial work. His monograph maintained that the roots of European culture were to be found in Egypt and Mesopotamia from where technology and political ideas had spread. Cultural innovation came about because wandering societies like the Indo-European or Semitic nomads distributed ideas from their homeland to other regions. Likewise, Kossinna had argued that the germs of German civilization had to be sought in the Indo-European expansion and the diffusion of ideas from one culture circle to another.

Along with Sollas' mélange of evolutionist and diffusionist thought, these two books could not have escaped the attention of the careful reader as they revealed some of the paradigmatic changes that took place in archaeology. The word 'culture' started to be used in plural and scholars no longer focused on 'the evolution of culture' but on 'the history of cultures' (Meinander 1981). Diffusion and migration replaced independent invention and parallel evolution as the key concepts for explaining cultural change. The interest in subsequent stages became an interest in adjacent *Kulturkreisen*; chronological preoccupations were joined by a geographi-

cal emphasis; and a new form of illustration, distribution maps, complemented the older chronological schemes. In the visual language of archaeology, the time-axis of the evolutionist diachronic scheme became an arrow which indicated wandering societies on the synchronic maps of the diffusionist. This shift from sociocultural evolutionism to cultural-historical archaeology was, of course, already adumbrated by Montelius' chronology of prehistoric Europe and Boas' historical particularism, but it was in the first decades of the twentieth century that the rupture was to become definite (Trigger 1989: 148-206).¹

This is most clearly seen in the early work of Myres' pupil at Oxford, the Australian archaeologist V. Gordon Childe. Already the title of his first archaeological monograph, *The Dawn of European Civilization* (1925), echoed Myres' *The Dawn of History* (1911). Childe was no longer interested in 'the common substratum' of humanity as described by Morgan, but focused on 'a peculiar and individual manifestation of the human spirit' in Europe (1925: xiii), thus testifying to the rise of particularism. His next book, *The Danube in Prehistory* (1929), dealt with the poorly understood region of Central Europe which was, however, the crucial passage way along which inventions from the Near East were brought to Western Europe. It contained Childe's classical definition of the culture concept as the constant recurrence of pots, implements, ornaments, burial rites and house forms which was 'the material expression of what would to-day be called a "people"' (1929: vi). This definition of the archaeological culture was identical to Kossinna's. Yet Childe was strongly aware of the potential political abuse of culture-historical archaeology, a danger which the Nazi appropriation of Kossinna's work would make painfully clear. For Childe, too, the spatial dimension was more important than the temporal dimension; he admitted that the culture concept was 'not necessarily a chronological concept' (vii). In the *Bronze Age* (1930) he elaborated this culture-historical approach, though more attention was given to social impact of technological innovations like metallurgy—a theme which would become the hallmark of his later work. As such, the work of Childe during the 1920s (which further included monographs like *The Aryans* from 1926 and *The Most Ancient East* from 1928) played a crucial role in the raise of culture-historical archaeology and the dismissal of classical evolutionism.

The consequences of this paradigmatic shift for the use of ethnographic analogy were profound. Sollas could only defend his source analogues by circuitous arguments which were supposed to show that the Tasmanians, Australians, Bushmen and Eskimos directly descended from their Palaeolithic counterparts. Now that the emphasis was no longer on universal stages of developmental growth, it became much more difficult to uphold the idea that one society was a fair representative of another. If prehistory is conceived of as an intricate web of migrating races which

1 Of course, Kossinna, Boas, and Montelius were rooted in the continental tradition where social evolutionism was clearly less popular than in Britain. However, it would be wrong to ascribe the divergence between them and the Victorian evolutionists strictly to a matter of nationality. This is clear from the fact that culture-historical archaeology rapidly won territory in British archaeology during the first decades of the twentieth century. It was first of all the growth of archaeological and anthropological data which forced a new perspective, and the approach hitherto favoured by several authors in Germany, France and Scandinavia proved more rewarding.

replace some and influence others, the only viable form of analogy becomes a direct-historical approach. Archaeologists in the first quarter of the century believed that ethnography could only shed light on the remote human past if there was evidence for historical continuity between the source and target analogues. Since few were prepared to follow Sollas' outrageous claims, the study of European prehistory (where urban and industrial revolutions implied a drastic break with the tribal past) had to be performed without ethnographic input. Kossinna proudly defended his '*rein auf archäologische Grundlage gestellte Volksforschung*', an ethnic inquiry strictly based on archaeological principles (1911: 2). In America, on the other hand, the alliance between archaeology and ethnology could remain intact since it was commonly believed that the native American Indian groups of the present directly descended from historical tribes. Individuals like Wedel and Strong could therefore use direct-historical analogies in their study of prehistoric Plains Indians (Willey and Sabloff 1980: 108-9; Charlton 1981: 137-9). Nevertheless, the sweeping ethnographic parallels of the evolutionist times never surfaced again but were replaced by minute one-to-one comparisons between prehistoric and present tribes.

Innovations in the Interbellum

Now it would be wrong to believe that this situation continued until the New Archaeologists of the early 1960s broke the deadlock. Despite the rhetorical writings of these angry young men in North America, the first half of the twentieth century was far less monolithic than their caricature suggested. Changes took place, both in theory and methodology. The later phase of the Interbellum was a period of intense theoretical debate and innovation, especially among British prehistorians. It is no coincidence that two leading journals in British archaeology originated in that era: *Antiquity* in 1927 and the *Proceedings of the Prehistoric Society* in 1935.²

Had traditional culture-historical archaeology been primarily concerned with filling in the time-space matrix of European prehistory (in America, they spoke of the 'ages and areas approach'), this objective was more or less met after a quarter century of research in the field and on museum collections. Indeed, by the early 1930s a rough outline of where and when the various cultures of prehistoric Europe had lived could be compiled, even if it still showed some hiatuses. In 1932, Burkitt and Childe compiled such comprehensive table chart for the *Antiquity* readership; it was the first in its kind (Burkitt and Childe 1932). Though it served as a basis for further refinement, it showed at the same time the outer limits of the culture-historical approach. This is well illustrated in the very first presidential address delivered by Childe at the Prehistoric Society only three years later, in 1935. Childe, who had by then become Europe's leading prehistorian, said that 'we shall continue to distinguish cultures and to assign each its proper place in a framework

2 After the 'coup' of Grahame Clark and a number of younger scholars, the somewhat obsolete Prehistoric Society of East Anglia was transformed into the Prehistoric Society; the Proceedings becoming their most immediate forum (Smith 1996).

of absolute chronology' but contended that this was not enough: 'It would be an old-fashioned prehistory that regarded it as its sole function to trace migrations and to locate the cradles of peoples.' Instead, once this is done, attention should be directed at a culture's 'changes in economic organization and scientific discoveries' (Childe 1935: 9-10). Childe, therefore, expanded Thomsen's typological three-age system to a functional-economic interpretation: each age was separated from the previous one by an economic revolution, a fundamental reshuffling of the forces and relations of production. This terminology betrays Childe's growing interest in Marxist archaeology, which was boosted by his 1934 travel through the USSR, though at this point he was more readily influenced by the functionalist anthropology of Radcliffe-Brown and Malinowski than by Marxism *per se*.

Even if not everyone partook in the enthusiasm for the Marxist movement and Russian archaeology (Grahame Clark, for example, named Soviet archaeology 'a department of Bolshevik propaganda' (1936: 248; cf. Tallgren 1937)), there was a growing dissatisfaction in Britain with culture-historical archaeology which was felt to be too sterile, too much concerned with pots instead of people. Instead of tracing the wanderings of abstract cultures on the basis of stylistic similarities and differences in ceramic decoration and other forms of ornamentation, some prehistorians increasingly focused on technological, economic and ecological themes. Apart from Gordon Childe's shift from a Kossinna-like culture-historical approach in the 1920s towards a functionalist archaeology of prehistoric economy and technology in the 1930s (which eventually developed into a full-blown Marxist archaeology in the 1940s and 50s), individuals like Grahame Clark and A.M. Tallgren urged to go beyond the 'cul-de-sac' of culture-historical archaeology in order to gain 'an understanding of social systems, of economic and social history, to the history of religious idea' (Tallgren 1937: 154). Bolshevik or not, technology, economy and ecology became the fields of attention for a number of British scholars who were acquainted with functionalist anthropology or Marxist social theory.

As a corollary of these broader aims, the thorny issue of ethnographic analogy was raked up again. In the very first issue of *Antiquity*, the editor O.G.S. Crawford (1927: 3) outlined the journal's policy as follows:

We shall not confine ourselves too rigidly within the conventional limits of archaeology. The past often lives on in the present. We cannot see the men who built and defended the hill-top settlements of Wessex, but we can learn much from living peoples who inhabit similar sites to-day in Algeria. From such, and from traditional accounts of the Maori forts we learn, by comparison, to understand the dumb language of prehistoric earthworks.

Crawford explicitly referred to the nineteenth-century evolutionists: 'this is the old anthropological method of Tylor and Pitt-Rivers and it has too long been neglected by archaeologists' (4). The article on Maori hill-forts in the same issue of *Antiquity* presented a detailed description of these monuments in New Zealand, preceded by the remark that they were 'strongly reminiscent of the British

hill fortress' (Firth 1927: 67). Yet how archaeologists could use evidence from the ethnographic present was not spelled out. The case study was not different from Sollas' parallel between Inuit houses and Scandinavian tombs.

Gordon Childe gave the issue more serious consideration in his classic *Man Makes Himself* (1936). He admitted that 'contemporary savages have just been described as living in the Stone Age to-day,' but added:

That does not justify the assumption that Stone Age men, living in Europe or the Near East 6000 or 20,000 years ago, observed the same sort of social and ritual rules, entertained the same beliefs, or organized their family relationships along the same lines as modern peoples on a comparable level of economic development.
(Childe 1936: 45).

Sollas' Bushmen, Eskimos or Arunta might have a similar tool technology and subsistence economy as prehistoric people had, this did not imply that their social and spiritual life had been arrested. The Arunta, for example, used only very simple utensils but had in the course of time developed extremely complex marriage and kinship rules. There was no causal link between a society's technology and its rituals, beliefs, and institutions. 'The assumption that any savage tribe to-day is primitive in the sense that its culture faithfully reflects that of much more ancient men is gratuitous', Childe opined (46). Ethnographic analogy could only be used 'as a mere gloss or commentary on actually observed ancient objects,' but anything more was 'illegitimate' (46-7). Childe repeated these ideas in his next widely-read book, *What Happened in History* (1942) where he said that ethnographic analogies could not 'disclose with scientific precision' the content of religious beliefs and social structures of Palaeolithic societies since 'that is unknowable' (45). He remained deeply sceptical about any serious contribution from ethnography (Trigger 1980: 75, 99).

Grahame Clark, the second key figure in the rise of functionalist archaeology, equally dealt with the problem. In *Archaeology and Society* (1939), probably the first book fully devoted to archaeological theory and methodology, he went back to the work of Nilsson, Lubbock and Pitt-Rivers but was far less enthusiastic about it than Crawford. 'The evolutionists made a grave error in treating human prehistory on the same level as the prehistory of animal species'; comparative ethnography should not be likened to comparative anatomy because 'modern savages have a history precisely as long as that of the most civilized peoples' (1939: 171). This awareness of the long histories individual cultures had gone through was very close to Childe's argument on the Arunta. In Clark's view, modern peoples could only be used 'with extreme caution and within well-defined limits, since one is otherwise in danger of assuming what one is after all trying to discover' (172), thus laying bare the circularity which had been imperceptible (or at least, harmless) to Victorian evolutionists. Ethnography could not give straight answers to the prehistorian's questions, but suggested plausible hypotheses and alternative ways of explaining patterns. It, too, was useful in imagining the stages of socio-cultural evolution:

If we can no longer follow the Victorian ethnologists in the stages they deduced from comparative ethnography, at least we may agree that in attempting to reconstruct those of prehistoric times from the contemporary evidence provided by archaeology we should do so with the insights to be gained from a study of living peoples at a broadly analogous stage of development. (Clark 1939: 173)

These insights did not so much come from descriptive ethnography, which was only useful for suggesting alternative hypotheses, but from social anthropology à la Radcliffe Brown and Malinowski, which ‘demonstrates how societies function and provides [the prehistorian] with a theoretical model on which to base his reconstructions’ (174). Functionalist social anthropology held a holistic view of society which was promising to the prehistorian because ‘by viewing the archaeological material and related evidences as traces of societies that once functioned as entities he may hope to understand it more fully’ (174). The whole could be known from its parts, even if these were just material.

The creative interwar work of Childe and Clark tried to break away from the sterile culture-historical approach of Kossinna and others. External migration and diffusion were not abolished (cf. Childe 1935), but the internal workings of prehistoric society received more attention than before. Both scholars stressed the need for understanding a society’s economic condition (Childe would eventually approach it from Marxist theory, Clark stuck to functionalist anthropology); both granted a restricted role to ethnographic analogy but kept the nineteenth-century comparative method at arm’s length. A similar dissatisfaction with culture-historical archaeology was voiced by A.M. Tallgren (1937), a Finnish scholar who functioned somewhat as a bridge between Soviet and European archaeologists. Against the *Kulturtkreis*-theory, he said that ‘the economic system as a whole was of more significance than nationality’; he, therefore, welcomed the Russians’ interest for economy but severely criticized their Marxist dogmatism (which made him *persona non grata* in Stalinist Russia, but loved by individuals like Clark). Ethnography helped him to debunk the basic tenets of culture-historical archaeology: modern tribes in Northwest Siberia, for example, showed that ‘material culture often cannot be equated with a “people”’ (1937: 156). Sometimes, people of one and the same culture used very different material repertoires, whereas on other occasions a particular material culture might crosscut ethnic divisions.

Anglo-Saxon culture-historical archaeology thought it could do away with ethnography, but ethnography showed the fundamental misconception of the culture-historical paradigm. Such lesson was also concluded in an article by Donald Thomson on Australian aboriginal camps. Solicited, encouraged and appreciated by Grahame Clark, it was published in the *Proceedings of the Prehistoric Society* in 1939. Thomson undertook what can be called the first ethnoarchaeological field study ever: by analysing the material remains of aboriginal sites after they had been abandoned, he did ethnographic fieldwork with an archaeological question in mind. That question concerned the impact of seasonality on the material culture of hunter-gatherers. Since seasonal changes strongly affected food supply, and as a consequence subsistence activity, occupation type and tool technology,

an innocent archaeologist seeing the varieties in material culture ‘would be led to conclude that they were different groups’ (Thomson 1939: 209). Material culture and ethnic culture did not overlap in a one-to-one way, Thomson suggested. Though his detailed study contrasts with the theoretical expositions of Childe and Clark, his article epitomizes the discussions of the 1930s: the critique on the culture-historical assumption which interpreted archaeological cultures in ethnic terms, the growing interest for ecological and economic factors (seasonality and subsistence), and the circumscribed function granted to ethnography. Once the scene which told prehistorians what the past had been, the ethnographic field had now become the place to tell what the past did *not* look like. Ethnography helped to criticize, to suggest alternatives, to give certain clues, but it did not present a ready-made picture of the past.

Marxism and folklore

While the 1930s had been a period of intellectual fermentation, the 1950s witnessed a growing sense of frustration among archaeologists. The interwar ambitions of going beyond the mere typological classification of artifacts were still regularly voiced, but it became less clear how this might be realized. Due to a drastically shaken composition of the academic demography, in archaeology, as in primatology, the war did not just impose an interruption of five years but of at least fifteen years. In order to understand the postwar context, let us first return to the two authors who had been responsible for the methodological and theoretical upheaval of the 1930s.

After the Second World War, Childe defended his Marxist position much more explicitly and elaborately than he had done before (McNairn 1980: 104–33; Trigger 1980: 125; Green 1981: 94–105). He grew increasingly interested in the issues of social evolution, economic history and technological progress and wrote about these in books like *Progress and Archaeology* from 1945, *History* from 1947, and *Social Evolution* from 1951, without, however, entirely denouncing the diffusionist stances from his earlier work. Childe’s thought, therefore, was an original interpretation of Marxism without the theoretical dogmatism so prevalent in the Soviet Union at the time. His interest for the writings of Marx and Engels inevitably brought him into contact with nineteenth-century social evolutionism, the very school culture-historical archaeology had set itself up against. In particular, the work of Lewis Henry Morgan, who had inspired Engels’ writings on the family, the state and property, exerted an increasingly important influence on Childe, though he found it very old-fashioned at first (Trigger 1980: 95). He eventually came to appreciate it better: ‘The sevenfold division adumbrated by Lewis H. Morgan and refined by Friedrich Engels, with his more comprehensive knowledge of European archaeology, is still unsurpassed’ (Childe 1946: 251). Childe borrowed Morgan’s notions of savagery, barbarism and civilization and interpreted these in economic terms: savagery was related to a foraging subsistence, barbarism implied an agricultural economy, and civilization emerged with urbanization. The Neolithic and urban revolution marked the transition from one stage to another. What Childe’s revolutionary view of prehistory came down to was in

fact an integration of Thomsen's three-age system (as it was refined by Lubbock), Morgan's evolutionist scheme and Engels' materialist reading of it, all this blended with a honest appreciation of the diffusionist explanation. Through Childe's synthesis, the Victorian scheme savagery-barbarism-civilization continued to be highly influential in twentieth century archaeology.

However, despite the evolutionist legacy, Childe remained deeply suspicious of ethnographic analogy which had once formed the cornerstone of Morgan's scheme. He criticized 'the shreds and patches' approach which interpreted the social structures of the Neolithic Swiss lake settlements in terms of Melanesian analogues simply because both 'lived in pile-dwelling, kept pigs, hafted stone celts by means of sleeves, and use the bow' (Childe 1946: 250). Such superficial and isolated similarities had no relevance whatsoever. Cultures could only be compared 'as organic wholes', only if they occupied 'the same relative position' in the evolutionist sequence (250). In *Piecing together the Past* (1956), Childe's exposé on archaeological interpretation, he argued that such comparisons could only shed light on the more material sides of human societies, i.e. technology and economy. Just as in 1936, he believed that similarity in these basic realms did not allow to predict similarity in ritual, ideological and political terms. This old idea was now rephrased with explicit reference to Marx. Whereas in good old Marxist tradition the socioeconomic and technological infrastructure was said to determine the spiritual superstructure, Childe regretted that many scholars 'confused "determines" with "causes"' (1956: 53). On the contrary, he said, Marx's postulate of determination implied 'anything but mechanical causation' so that a society's spiritual culture could not 'be inferred with any confidence from the technology' (53). Whereas Childe accepted the technological similarity between source and target analogues, he denied that there was an unambiguous causal relation linking technology to other parts of society. Consequently, 'no existing society today is so exactly representative of any past society [...] that its rituals or social institutions can provide precise and reliable explanations of the more puzzling relics and monuments recovered from the past' (55).

The use of ethnographic analogy was thus relegated to the more mundane aspects of archaeological reconstruction, but even then Childe believed these afforded 'only clues in what direction to look', nothing in the sense of explanation (49). Childe further maintained that the most promising results could be booked if the modern society lived in a comparable environmental context or, better still, if there was historical continuity between the analogues. Historical continuity increased the relevance of the observed similarity between source and target analogues as it pointed to a shared cultural tradition. This was even possible in an European context: 'For interpreting archaeological relics from northern Europe analogies in contemporary folk-culture of that area are more useful and more reliable than any parallels, even though they seem more exact, from Tierra del Fuego or British Columbia' (48). In the absence of clear historical continuity, analogies were 'always somewhat suspect' (48). Childe used such historical parallels with rural life in Scotland and the Hebrides in his interpretation of the Neolithic

settlement of Skara Brae in the Orkneys which he had excavated during the Interbellum (Trigger 1989: 263).

This enthusiasm for folklore studies as a means of elucidating later European prehistory was an idea fervently defended by Grahame Clark. In a *liber amicorum* presented to O.G.S. Crawford—the man who had withheld a Victorian-like enthusiasm for ethnographic parallels—Clark still cautioned against too sweeping comparisons and warned for circular reasoning. He found that claims, like the ones made by Sollas', which went 'so far beyond the available evidence often daunt rather than stimulate' (1951: 55). On the other hand, the latter's work had also a great advantage over the nineteenth-century evolutionists: 'Sollas was surely right to imply that remains of extinct cultures can only be interpreted with any certainty through modern analogues, if a continuous historical sequence can be demonstrated between them' (55). Clark believed that the contemporary folk-culture of the most rural parts of Europe such as 'the Celtic fringe of Britain, the Scandinavian countries, the Alps, the Balkans and the Mediterranean basin' often showed continuity of settlement 'since neolithic and to some extent since mesolithic times [...] down to our own day' (57, 55). The basic premise was simply Tylorian: 'Wherever civilization has developed, there are liable to be survivals from earlier times in the culture of the countryside, from which the prehistorian can profit' (57). The study of folk culture could not only help by interpreting objects and excavated features otherwise enigmatic, it could also clarify the subsistence economy of prehistoric communities.

Interpreting the organic remains of the rich waterlogged Mesolithic site at Star Carr, Clark thus drew in evidence from the least industrialized parts of Europe to enhance his reconstruction of prehistoric activities (Clark 1954). Although he equally looked at the material culture of the more remote contemporary Eskimos, just like Childe, he kept an outspoken preference for analogies where there was a demonstrable historical continuity. Such continuity was more weighty than environmental parallels because even if ecological surroundings were similar, 'great diversity of cultural expression may be found' (1953: 355). In his classic *Prehistoric Europe* (1952) he repeated that one had to avoid 'the indiscriminate seeking of parallels among cultures far removed in time and space and with which no continuity of tradition can be traced' (1952: 3). Consequently, when one was studying the older phases of the European Stone Age, no historical parallels were available so that 'one is forced to go outside' (Clark in Tax et al. 1953: 232). For Clark this meant outside Europe; for the American scholar Slotkin this meant that when investigating pre-sapiens populations the study of nonhuman primates might be more profitable than the vague generalizations about contemporary human foragers (1952). While ethnographic analogy had once been crucial for understanding the oldest Stone Age, it was now restricted to the more recent phases of prehistory through the use of folk studies. The reliance on European folklore was already intimated by individuals like Nilsson and McLennan, but Clark (1951; cf. 1974) revived it in the context of the mid-twentieth-century interest for ecology: comparisons were to be made within the same environment and when there was historical continuity between source and target analogue. Similarity still determined



Figure 8. Clark compared salmon traps currently used in Sweden with osier traps from the Danish Stone age. Analogy could only be applied on a technological level when ecology and economy were similar. This was the exact opposite of Tylor (figure 7) for whom similar technology was enough to infer a similar level of progress. Analogy in the first half of the twentieth century was more careful to balance the weight between premises and conclusion (Clark 1952: plate II)

the success of an analogy, but instead of strict formal resemblance between material culture items (as in the nineteenth century), ecological parallelism and historical continuity were now regarded as the most relevant criterions. As with Childe, Clark's analogies were confined to the more material sides of archaeological reconstruction: for instance, Danish prehistoric objects like a Mesolithic fish trap, a Neolithic canoe, and containers in birch wood were juxtaposed to recent examples from Finland in order to illustrate their function and production (figure 8). Clark stressed, however, that where 'comparative ethnography can prompt the right questions; only archaeology [...] can give the right answers' (1953: 357).

Clark's interest for folklore analogies formed part of his more general desire 'to break away from the object-fetishism' which still plagued most of the culture-historical archaeology of his time (Clark 1974: 40). Indeed, the opening sentences of *Prehistoric Europe* (1952) were very clear about this: 'Archaeology is often defined as the study of antiquities. A better definition would be that it is the study of how men lived in the past.' Clark's ambition was first and foremost to find out how prehistoric cultures adapted themselves to their surrounding environments by means of economic organization. Viewing each culture as 'the product of an equation between social inheritance and the various elements in the biome' (1953: 351), he laid out the foundations of the ecological approach which was to become so prosperous in archaeology. As he considered Marxist theory 'equally out of date' as the speculations of the Victorian sociocultural evolutionists, attention for social issues was rather minimal (1953: 346). Not surprisingly, Gordon Childe's reaction after reading *Prehistoric Europe* was: 'Yes, Grahame, but what have you done about Society?' (Clark 1974: 48). In the absence of an agreed upon social theory, interpretation beyond the technological and economic level became problematic, especially as ethnographic or folklore analogy was not believed to be of any help here.

Postwar pessimism in Britain

Clark's position exemplifies the stalemate which characterized so much of British archaeology in the 1950s: the ambition to do more than culture-history archaeology, and the simultaneous frustration about the impossibility of reaching a genuine understanding of prehistory in social terms. This is not to say it was a impoverished decade of archaeological research. On the contrary, with individuals like O.G.S. Crawford, Christopher Hawkes, Mortimer Wheeler, Cyril Fox, next to Grahame Clark and Gordon Childe, and, in a later phase, Stuart Piggott and Glyn Daniel on the scene, British archaeology went through one of its most colourful and active episodes. It was a period of numerous and spectacular excavations executed according to the best standards since Pitt Rivers; scientific methods like pollen analysis, radiometric dating, and chemical determinations of the origin of raw materials were increasingly drawn upon; and photography, especially aerial photography, made its entrance as an important tool for surveying. Yet all these were methodological improvements, not theoretical ones. They dealt with the reconnaissance, the recovery and the analysis of archaeological remains, not with the problem of how to interpret them. Despite the disenchantment with

the culture-historical paradigm, no convincing alternative ways of reconstructing past societies were proposed (apart from Childe's, but whereas his earlier, culture-historical work had been greatly influential, few British scholars were at the outset of the Cold War willing to accept the Marxist implications of his later work).

While establishing the culture-historical framework was an essential task of the archaeologist, all scholars accepted it could not be the only one. Christopher Hawkes, for instance, found that '“when and where” archaeology should be a means to a further end' (1954: 156). Grahame Clark was concerned with 'reconstructing rather than merely classifying the material traces of the past' (1952: 3). Mortimer Wheeler noticed that for a long time 'we have [...] been preparing time-tables; let us now have some trains' (1954: 215). And he added his famous dictum that 'the archaeologists is digging up, not *things*, but *people*' (1954: v). This was all well and good to say, the question remained how such objective could be reached.

It was further complicated by the general and widespread belief that human beings were historically unique entities which could not be treated in the same terms as the rest of nature. This was a point stressed by nearly all archaeologists from the postwar decade. Childe mentioned 'the distinctively human needs' (1956: 49) which set *Homo sapiens* apart from the other animals. Clark noted that modern archaeologists studied 'the multitude of unique events conditioned by cultural and even personal factors' (1953: 354), in contrast to the Victorian evolutionists who had precisely failed 'to appreciate the unique historical character of cultures'. Consequently, the uniformitarian assumption at stake in comparative anatomy and geology could not be applied to comparative ethnography: 'The task of reconstructing the life of prehistoric communities is inherently more difficult and hazardous than deducing the behavior of Pleistocene glaciers from observations of existing glaciers obedient to immutable laws' (354). Similarly, Wheeler was 'not over-readily tempted to equate development of human institutions with the normal processes of organic evolution, to Darwinize human “progress”' (1954: 207). In his unparalleled style, he wrote: 'We need not close our eyes to Man-the-Jelly-Fish or Man-the-Whole-time-Food-gatherer in order to believe in Man-with-Time-to-think-between-Meals, in Civilized Man, but the last is surely, of overriding importance' (208). Man was unique—even to the point of being 'noble' (Wheeler 1954: 209)—, cultures were diverse, and history was complex, these were axioms of postwar archaeology. Many would have agreed with Clark in his reflection on the 1950s: 'People are more complex beings than fossil mammals and archaeology is that much more difficult' (1974: 54).

Much more difficult. Indeed, stressing human uniqueness had the consequence that generalizing statements could only be made on the most mundane levels. Since every culture was a unique historical entity, since humans did not obey to laws, since economy did not dictate the rest of society, little could be said that was cross-culturally applicable. We have already seen how Childe and Clark believed that ethnographic analogy could only be employed on the technological and economic level. In a more general sense it was believed that these were the realms which the archaeologist could readily access; anything above it was

harder to comprehend or simply beyond the grasp of the investigator. This was precisely the idea of Christopher Hawkes' famous ladder of inference, i.e. that there was a hierarchy of inferential accessibility which went from material techniques, over subsistence-economics and social organization to ideational themes like religious institutions and spiritual life (1954: 163). It says something about the *Zeitgeist* that similar hierarchies were independently formulated at the same time (cf. Childe 1946: 249; Smith 1955: 6) and that Hawkes' version was eagerly adopted by others (cf. Piggott 1959). Even if Hawkes' ladder has become part of every undergraduate training in archaeology, it is commonly forgotten that it, too, was formulated within the discourse on human uniqueness versus human animality. 'What is this climax?' Hawkes wondered about his ladder:

It is a climax leading up from the more generically animal to the more specifically human. [...] So the result appears to be that the more specifically human are men's activities, the harder they are to infer by this sort of archaeology. What it seems to offer us is positively an anticlimax: the more human, the less intelligible. (1954: 162)

This, then, was the paradox of postwar British archaeology: firmly given to reconstructing humans in the past, its very definition of humanity (as uniqueness) inhibited the realization of such reconstruction. How was one to remedy this situation?

A first option entailed consulting anthropology, according to the adage 'if you're stuck in the prehistoric past, go and check the anthropological present.' This is what O.G.S. Crawford suggested. Thirty years after his editorial in the first issue of *Antiquity* he still wholeheartedly supported 'this business of using the present to interpret the past' (1960: 226). In his methodological work *Archaeology in the Field* (1960), he devoted two chapters on the employment of anthropological data in archaeological contexts. Although he deplored the anthropologists' lack of attention for material culture ('Too often we see page after unreadable page on the systems of relationship, and little or nothing about the pots' (222)), he praised the high potential of ethnographic facts. Anthropology could 'greatly help' in the 'imaginative process' which all archaeological interpretation seemed to require (224). After a travel through Sudan, Crawford was convinced that 'people in many regions are still living a life which is "prehistoric" in the European sense' (226) so that Sudanese hut structures, markets, house types, pottery helped 'to see the past in the present' (231). For example, since in Sudan a great number of hut structures did not imply a large population, the Dartmoor settlements in Devon might have equally been thinly populated, he estimated.

Few were, however, ready to join Crawford's enthusiasm for such nearly nineteenth-century-like analogies. Apart from Clark's and Childe's preference for folk analogies, Hawkes called for 'a sound critique of the comparative method in its reasoning' (1954: 168) and De Laet (1957: 95), whose methodological essay on archaeology was translated into English, asked for 'the greatest caution [...] whenever ethnology or folklore is called upon'. Unlike the Victorian evolutionists who reasoned from the supposed similarity between savages, postwar archaeologists

only found cultural variability. Childe stressed that a study of ‘the simpler peoples of today reveals the endless diversity of human behaviour’ (1956: 55). And Smith wrote:

It used to be thought that studies of surviving primitive peoples would provide the necessary analogies for interpreting prehistoric societies; but in the event the extension of ethnological studies has only served to show what an incredible variety of codes of behaviour in fact actuate human conduct. (Smith 1955: 4-5)

Anthropology, according to Smith, made interpretation even harder instead of easier.

A second means of accessing ‘humans in the past’ consisted of improving the empirical and methodological conditions of the archaeological research. Put simply: one needed more excavations and better methods. The postwar decade was characterized by an increasing number of excavations, preferably at such rich sites like Star Carr (excavated by Clark), the Somerset Levels (where Godwin worked) and Maiden Castle (unearthed by Wheeler). Waterlogged sites where organic tools, faunal and floral remains were exceptionally well preserved became popular. Clark, for example, admitted that he undertook ‘a prolonged search’ in order to find a promising site like Star Carr (1974: 50). Such preferences for rich sites would later be criticized and balanced by off-site studies, surveys and excavation of minor sites. In the 1950s, however, archaeology was ‘primarily a fact-finding discipline’ (Wheeler 1954: 200); good archaeology came from good sites. It suffices to look at the titles of some books to realize the centrality accorded to excavation within the archaeological enterprise: *Field Archaeology* (Atkinson 1946), *Archaeology from the Earth* (Wheeler 1954), *Still Digging* (Wheeler 1955), *Archaeology in the Field* (Crawford 1960), and so on. Apart from romantically glorifying the dig, such books reveal the eminence of field methodology and techniques. It was commonly accepted that the excavator and the archaeologist should be one and the same person (De Laet 1957: 77; Piggott 1959: 14); in a very literal sense, doing archaeology came down to ‘digging up the past’ (Woolley 1930). Most theoretical books were therefore in the first place treatises of field methodology. The American scholar Walter W. Taylor (1948: 43) even contended that archaeology *per se* was ‘no more than a method and a set of specialized techniques [...]. The archeologist, as archeologist, is really nothing but a technician.’ Probably at no other time in the twentieth century were the standards of archaeological excavation as high as in the 1940 and 50s, yet at no other time was the reconstruction of past societies felt to be so problematic. Clark realized, however, that the wealth of evidence was not the sole criterion: ‘even if a complete range of material equipment of a prehistoric group could be recovered [...] the problem of interpreting this correctly would still remain more complex than is always allowed’ (1951: 64). According to him, the emphasis on field methods could give a false impression of inferential confidence:

The very concentration on the material evidence, which distinguishes modern archaeology, the careful scrutiny, the accurate description and illustration, the circumstantial method of publication, all tend to make us feel that the conclusions reached are more firmly based than they often can be. (1951: 63)

The archaeological database could equally be enhanced by calling in various sorts of specialisms and what used to be called ‘scientific techniques’. Grahame Clark in particular was a great supporter of this new tendency because it allowed ‘to extract the fullest information’ (1952: 2). Petrological determinations of the origin of raw material, chemical and spectographic analyses of bronze objects, archaeobotanical and palynological investigations, zooarchaeological study of faunal remains, all these approaches were welcomed as they pulled archaeology away from the mere cataloguing of tools to a fuller understanding of subsistence, diet, climate, and biome. However, not everybody shared this optimism; many felt that a single-minded dependence on natural science might in the long run jeopardize human uniqueness and dignity. Wheeler was ‘regretting the prospect of archaeology passing wholly into the hands of the biologist and the technician’ (1954: 210-1). Smith warned against ‘the danger that archaeology might come to be equated simply with a competence in handling a set of techniques’ (1955: 3). The longest and most famous attack against such scientism was Jacquetta Hawkes’ paper ‘The proper study of mankind’ published in *Antiquity* at a moment that ‘our technological Frankenstein’s monster’ was already out of control (Hawkes 1968: 262). ‘We must remember,’ she wrote, ‘that all our ingenious devices, all our exact measurements and statistical analyses, are of no value in themselves’ (262). Indeed, her entire paper aimed at ‘preventing the scientific and technological servant from usurping the throne of history’ (255). Whereas human uniqueness had only just been saved out of the claws of a biological approach, the new scientific creed risked to entail a new form of ‘dehumanization’ (260). Biological substance and spiritual being, no one described this ambiguous approach to humanity better than Mortimer Wheeler (1954: 201) when he wrote: ‘Man is in some sense the casket of a soul as well as five-shillings-worth of chemicals.’

In line with this ‘humanistic approach to the study of antiquity’ (Wheeler 1954: 209) there was an insistence by some scholars on the more noble, the more civilized moments of our past. Wheeler urged British archaeologists to concentrate upon ‘the riper achievements of Man as social animal’ (1954: 212); J. Hawkes regretted ‘the relative neglect of the higher human achievements’ in recent archaeology (1968: 260). The attention should be less on the scraps and pieces of humanity’s dismal beginnings, but on its monumental attainments at sites like Mohenjo-Daro, Nippur and Stonehenge. All this came down to an appreciation of humans as unique, symbolizing, and above all historical beings.

A third strategy adopted to increase our understanding of the past was, therefore, firmly rooted in the historical tradition. Surely, more excavations, better excavations, newer techniques, all this had resulted in larger amounts of valuable evidence, yet how to interpret this evidence was another question altogether. The reliance on natural science methods notwithstanding, archaeologists insisted that what they were doing was history.³ It is important to bear in mind that scholars like Wheeler, Childe and Hawkes did indeed descend from the historical-philological tradition. The qualities needed for archaeological interpretation were those of the historian: Wheeler named ‘constructive imagination’ and ‘intuitive comprehension’, the archaeologist being ‘something of an artist’ (1954: 201). Likewise, Crawford relied on his ‘creative imagination’, ‘imaginative thinking’ and a special ‘attitude of mind’ (1960: 224, 231). The American archaeologist Thompson spoke of the ‘individual sensitivity’, the ‘intellectual ability’ and ‘the “feel” which an investigator has for the material’ (1956: 327, 328, 332). Even if such notions might sound rather vague, they were in fact the warmly embraced tenets of historical interpretation in the contemporary hermeneutic work of the philosopher-annex-archaeologist R.C. Collingwood. In his influential *The Idea of History* (1946: 282) he argued that ‘the historian must re-enact the past in his own mind.’ Archaeological interpretation was not a set procedure of mechanical inferences, it required certain skills such as intuition and empathy.

Another necessary skill was literary giftedness. If there are no set procedures for interpreting archaeological material, imaginative writing becomes the last (and often most elegant) resort. It is no coincidence that some of the finest writers in archaeology of the twentieth century (like Glyn Daniel, Stuart Piggott and especially Mortimer Wheeler) worked within this humanistic framework. For Wheeler, whose eloquent prose has been repeatedly quoted here, the spade was never ‘mightier than the pen; they are twin instruments’ (1954: v). He was very much concerned with how words could vivify the past; this in fact was what ‘the creative act of reasoned imagination’ came down to (4): facts had to be ‘infused with rational intelligence’, so that archaeological writing was more than ‘prosaically revealing and cataloguing our discoveries’ (4). Jacquetta Hawkes, too, as a late representative of this humanistic ideal, bitterly observed in 1968:

If there is anybody under the age of thirty or so producing historical writing of the quality and humanity of the work of the young Gordon Childe, Mortimer Wheeler, Christopher Hawkes, Stuart Piggott, or even in his more austere way, Grahame Clark, I have failed to see it. (1968: 256)

Interestingly, only the young, pre-Marxist, culture-historical Childe is appreciated. Though Wheeler was well aware that he represented ‘the end of an active generation’ (1954: 215) of humanistic scholarship, historical orientation and

3 Trigger (1982) has noticed that the orientation of British prehistoric archaeology towards history was also given in by the sort of social anthropology which prevailed at the time: structural functionalism regarded societies as stable entities so that change, the process archaeologists were most interested in, was nearly viewed as ‘pathological’.

literary erudition, J. Hawkes did not seem to realize that in a fundamental way 'the times were a-changing.'

Anthropological inspiration, methodological and empirical refinement, historical intuition, and imaginative writing, if all these devices sought to meet the requirement 'to dig up people rather than mere things', they were not necessarily compelling to everyone so that a profound scepticism could linger on in the discipline. This was the extreme position put forward by M.A. Smith in her paper on 'The limitations of inference in archaeology' which was read at the Prehistoric Society (1955). She noted that archaeologists in recent years had reasserted the discipline's 'essentially human aspect against a too persuasive "scientific" approach' by trying 'to re-create the past' (3, 4). Smith outlined the difficulties involved with such ambition. Deeply sceptical of any assistance of anthropology, she presented an epistemological pessimism which went further than Hawkes' hierarchy: 'it is a hopeless task to try to get from what remains to the activities by argument' (6). One could infer some aspects of prehistoric economics on the basis of the available evidence, but anything else was 'nothing less than a demand for logical alchemy' (6). Smith denied that there was a causal correlation between archaeological statics and dynamics, there was 'logically no necessary link' between the two (6). As a consequence, her text was littered with phrases like 'we do not get very far', 'we can never know', and 'it would be impossible to understand' (4, 6, 5). Smith, therefore, argued that 'unobtainable ends cannot be the proper ends for any subject'; it was even morally wrong:

Since historical events and the essential social divisions of prehistoric people don't find an adequate expression in material remains, it cannot be right to try to arrive at a knowledge of them in archaeological interpretation. (7)

It may already be noted that the New Archaeologists, with their enormous epistemological optimism, regarded this as an inimical call to arms. Binford even used the above quotation as the ironic motto of his first book. Smith, however, preferred not to say that 'archaeology "re-creates", or "reconstructs" at all; it merely recovers what it can. That of itself is a sufficient programme of research' (7). With this statement we have come full circle: from the wish to go beyond the mere study of objects, over the inherent difficulties to do so, we have now arrived at the humble realization that archaeology should be confined to the objects proper.

Needless to repeat that the role of ethnographic analogy was minimal throughout. At best it had to satisfy itself with merely stirring the historical imagination (as Crawford suggested), but in general it was confined to the most mundane aspects of culture (as Childe explained), it could only be used when there was historical continuity (as Clark employed) and it was of no use for the more remote phases of human prehistory (as Slotkin argued).

The situation in the United States

Reading through American publications of the postwar decades, the differences with what was happening in Britain seem at first sight enormous. Contrary to the arabesque prose of Wheeler and J. Hawkes, the writing style of authors like Walter Taylor, Gordon Willey, and Raymond Thompson was much more terse and analytical. While British scholars aligned themselves with the field of history, American archaeologists sided with anthropologists—just think of the famous catchphrase ‘American archaeology is anthropology or it is nothing’ (Willey and Phillips 1958: 2). Taylor contended that when an archaeologist was interpreting his evidence he was ‘an anthropologist who works in archaeological materials’ (1948: 43), while Willey and Phillips (1958) sought to integrate American archaeology into general anthropological theory. Archaeology was one of the four fields Boas had defined for anthropology (next to linguistics, physical anthropology and cultural anthropology) so that the conceptual links were much closer than in Europe with its mutual incomprehension between prehistory and anthropology (Trigger 1982). Theoretical debate in archaeology, for instance, was conducted in anthropological publications like *Anthropology Today* (Kroeber 1953) and the *Southwestern Journal of Anthropology* (which played an increasingly prominent role for archaeological discussion during the 1950s and 60s).

However, there was no yawning gap between the Old and the New World as archaeologists in Britain and North-America did communicate with each other (cf. Clark 1953; Willey 1953; Hawkes 1954). Individuals like Childe and Hawkes published important theoretical papers in American journals like the *Southwestern Journal of Anthropology* and *American Antiquity* (Childe 1946; Hawkes 1954). And, more importantly, some of the fundamental preoccupations with theory and methodology were alike on both sides of the Atlantic.

First of all, the incipient discontent with culture-historical archaeology was as outspoken as in Britain. Though much of postwar American archaeology dealt with taxonomy, stratigraphy, seriation, typology, and classification, Braidwood’s dictum that one should not lose sight of ‘the Indian behind the artifact’ was widely supported. An innovative thinker like Walter Taylor argued at length that mere culture history could not be enough; his influential essay *A Study of Archaeology* (1948) was in fact one long critique against such myopic vision of the discipline’s aims. Gordon Willey agreed and said that next to a ‘historical’ objective which entailed ‘the descriptive identification and space-time arrangements of data’, archaeologists should also focus on a ‘processual’ objective, which was ‘the functional or use identification of interpretations of data’ (1953: 363). Later this second level was even conceived in still broader terms, not just functional interpretation: ‘we are no longer asking merely what but also how and even why’ (Willey and Phillips 1958: 6). At the influential Wenner-Gren meeting of 1952, Willey said: ‘Contextual reconstruction or descriptive integration, the translation of cultural and human fossils into an image of life, is now the primary problem area of archaeology’ (in Tax et al. 1953: 251). This was exactly the programme that Clark and Wheeler had been advocating.

Also similar to currents in British archaeology was the awareness that such reconstructive ambition was more difficult and necessarily implied a ‘subjective element in archaeological reference’ (Thompson 1956). Thompson argued that every interpretation consisted of an ‘indicative’ and ‘probative’ phase (hypothesis-formulation and hypothesis-testing as it would now be called) and that a subjective element was injected into both. Again, the quality of the interpretation depended on the special skills of the researcher: ‘The individual investigator with his unique combination of interpretative skills provides the only possible means for the reconstruction of the cultural context of an archaeological collection’ (331).

In terms of ethnographic analogy, Taylor who discussed nearly every aspect of Americanist archaeology was surprisingly brief on it as he found it ‘possibly gratuitous’ (1948: 171) to remark that such ethnographies were useful. Like Clark, he urged to go further than the interpretation of the use and function of artifacts to a wider appreciation of the archaeological cultural context. Willey introduced the terminological distinction between a ‘general comparative analogy’ and ‘specific historical analogy’, the former involving ‘cultures of the same general level of technological development, perhaps existing under similar environmental situations’, the latter involving ‘a presumed or reasonably demonstrated cultural continuity from prehistoric to historic times, on into a living ethnology in the same area’ (in Tax et al. 1953: 229). Willey indicated very clearly which of the two he preferred: it was with the specific historical analogy that we were ‘on stronger ground for interpretations’ (229) and where ‘the most immediate progress’ (252) was to be expected. This was identical to Clark’s and Childe’s preference for analogies drawn from folklore studies where there was demonstrable historical continuity. It too continued the long-standing prewar tradition in American archaeology to compare excavated relics with contemporary native descendants. Throughout the first half of the century, the direct-historical approach was regarded as the strongest form of analogy as it increased the amount of observed similarity.

However, Thompson attributed a far greater role to ethnographic analogy than that of tool for interpretation. In his opinion, the ethnographic present could be used to *verify* the archaeologist’s hypothesis about the past. ‘The artifact-behavior correlation’ one suggested for the past must be ‘a common occurrence in ethnographic reality’ (1956: 329). Drawing on the logic of John Dewey, he presented a detailed mechanism of analogical reasoning by formal similarity:

What actually happens is that [the investigator] compares an artifact type which is derived from archaeological data with a similar type in a known life situation. If the resemblance in the form of the two artifact types is reasonably close, he infers that the archaeological type shares the technique, behavior, or other cultural activity which is usually associated with the ethnographic type. (329)

This was one of the first times that the logic of analogy was explicitly considered. If Willey thought that ethnographic analogy was a matter of ‘take it or leave it’ and that one could do without, Thompson held that ‘archaeological inference is impossible without recourse to analogy’. These proved to be prophetic words.

Cultural continuity

The period between the heyday of Victorian evolutionism and the rise of neo-evolutionism in archaeology, roughly speaking the half century between 1910 and 1960, is traditionally described as an essentially Kossinna-like culture-historical archaeology where ethnographic analogy had no say. Though this was the case at the extremes of the period, i.e. at the start with Kossinna and the young Childe and at the end with Hawkes and Smith, in the intervening period ethnography did play a considerable role in archaeological interpretation, albeit more restrictedly than in Victorian anthropology of New Archaeology.

Contrary to the holistic projections of Morgan, Tylor and Sollas, ethnographic analogy was now generally limited to the clarification of more humble issues like tool function and subsistence. Non-modern societies were no longer regarded as representing universal stages of social, legal, religious, and intellectual development but as cultures with particular technologies and economies living in a specific social and ecological environment. In the absence of a hierarchy of sociocultural complexity, analogies were now considered to be safest when societies in similar technological and environmental contexts were compared, preferably when they demonstrated historical continuity. As a consequence, the direct-historical approach was advocated by scholars with the most diverse theoretical positions. No matter whether one's interest was in Marxism, ecology, or culture-history, cultural continuity was praised as the best warrant for analogical inference.

At the heart of the direct-historical approach was a confidence in similarity as the base for analogy. Next to resemblances in ecology, economy and technology, evidence for long-term relations between the present and the past was considered to top the amount of observed similarity between source and target. Indeed, the purpose of archaeologists was to find a living analogue which approximated the past society under consideration. The degree of proximity was seen as the decisive yardstick for evaluating source analogues. The closer in time and space the analogue was found, the better.

Such insistence on similarity obviously recalls the nineteenth-century obsession with that criterion. However, there are important differences. Even if both forms of analogical reasoning relied on the number of resemblances, the meaning of similarity depended on the broader, often implicit theoretical framework one was working in. In the Victorian sense similarity was defined in terms of relative position on the sociocultural ladder. In the sense of culture-historical archaeology it was intimately related to the concept of archaeological culture as a space-time entity developing within a given context of social, historical, and ecological parameters. For a Victorian evolutionist, similarity meant similarity of stage, regardless of 'place on the map or date in history' as Tylor argued. For a culture-historical archaeologist, it meant the exact opposite: spatial proximity and historical continuity were the very determinants of the analogy's quality. It was precisely this version of relevant similarity that started to be challenged in the early 1960s.

The dilemma of the New Archaeology

The movement in North-American archaeology which emerged in the early 1960s and came to be known by the name of the ‘New Archaeology’ initially maintained a rather ambiguous attitude towards the use of ethnographic analogy. Whereas on the one hand it tried to move away from the strict confines of the previously popular direct-historical approach, it did not wholeheartedly embrace a general-comparative approach on the other hand either. Attracted to the neo-evolutionist arguments put forward by the anti-Boasians in cultural anthropology, Lewis Binford, the broadly acknowledged leader of the New Archaeology, sought to align archaeology more closely with anthropology. The aim was to make the discipline more scientific, to provide it with a methodology far more rigorous than hitherto seen, and to supply it with reasoning in terms of logical deduction rather than intuitive induction. Yet this entailed a fundamental dilemma: the most obvious procedure archaeology could draw upon to benefit from work in anthropology was by analogy. But since this was an inductive procedure, archaeologists were at pains to reconcile it with their deductive ambitions. There was, so to speak, a clash between the New Archaeology’s association with neo-evolutionist anthropology on the one hand, and its inspiration in positivist philosophy of science on the other. As a consequence, the notion of ethnographic analogy remained profoundly ambivalent throughout the 1960s. While the conviction that much could be learnt from contemporary non-industrialized societies drove several archaeologists to the ethnographic field, at the same time scholars constantly avoided a direct application of the insights gained to specific archaeological contexts. How this dilemma was circumvented can only be understood if we first direct our attention to some of the revolutionary publications which set the New Archaeology in motion.

The new analogy and the New Archaeology

Two papers from the early 1960s were instrumental in changing the climate of opinion on the aims and methods of Americanist archaeology as it had been canonized by Willey in the late 1950s. They were Robert Ascher’s ‘Analogy in archaeological interpretation’ (1961) and Lewis Binford’s ‘Archaeology as anthropology’ (1962). Published in the discipline’s leading journals at the time—*Southwestern Journal of Anthropology* and *American Antiquity* respectively—they were widely distributed, widely read and widely commented upon.

Ascher’s paper started with the observation that whereas the archaeology of the 1950s had been greatly successful in the formulation and refinement of concepts, the gathering and processing of data, and the crafting of general syntheses, hardly any progress had been made in terms of interpretation—this was precisely the interpretative difficulty which had also been notified by the previous generation. Ascher cited the pessimistic publications by Hawkes, Smith and Thompson and regretted that they were ‘not undertaking interpretation at all’ (1961: 321). He concurred, however, with Thompson that ‘the most widely used of the tools of archaeological interpretation is analogy’ (317) but differed from him in the importance attached to personal competence. According to him, analogies, when

properly used, could take archaeological interpretation beyond the level of individual sensitivity, although this was no easy business. Ascher was well aware of ‘the fact that analogy in archaeological interpretation has suffered chronic ambiguity since the nadir of classical evolutionary simplicity’ (322). Ascher explicitly signalled the many anachronisms in the arguments of Sollas and was ‘anxious to avoid the mistakes of the early evolutionary school’ (319). Aware of the problems involved with analogical reasoning yet refusing to accept the resigned pessimism of the previous generation, Ascher’s ambition consisted in ‘placing analogy on a firmer foundation’ (322).

Following Willey, he distinguished between an analogy based on historical continuity (encompassing the direct-historical approach in the New World and the folk-culture approach in the Old World) and one based on a comparison of unrelated cultures ‘which manipulate similar environments in similar ways’ (319). The latter he called ‘the new analogy’ (although he traced its history back to Childe and Clark) and, contrary to Willey, he was firmly in favour of it: it enhanced our interpretation for the tract not covered by historical continuity, a tract ‘consisting of over ninety-five percent of human history and a large proportion of the globe’ (319). Though Ascher acknowledged Clark’s folk-analogies, he was more enthusiastic about the latter’s employment of an Eskimo parallel. In his interpretation of Star Carr, Clark had inferred that women must have been present because there was evidence of skin-working, a task which was commonly performed by women among the Eskimos of North-America and Greenland. Star Carr was thus more than an all-male hunting camp, but represented the remains of an entire community. Ascher praised this argument ‘as an excellent example of the new analogy’ (320). In the absence of historical continuity, the new analogy proceeded by considering potential source analogues and by systematically eliminating the least productive ones in order to select ‘the best solution’ (322). Elimination happened on the basis of economy, of distance between source and target (temporal, spatial and morphological), and of ‘closeness of fit of the relationships between forms in the archaeological situation with relationships between forms in the hypothesized analogous situations’ (323). What Ascher did was select the best available source by means of the number of formal similarities it presented to the source. Similarity, i.e. closeness of fit, determined the validity of the analogy. In this sense, he was perhaps more nineteenth-century in his outlook than he wished to acknowledge. The important aspect to remember, however, was that he broke the

interpretative impasse archaeology had become entrenched in during the 1950s. Against Smith, he said that his ‘clear systematic approach’ required ‘no touch of alchemy’ (323). Against Hawkes, he said that an interpretation beyond the technical and subsistence-connected level was ‘only apparently remote’ (324).

If a certain epistemological optimism surfaced in Ascher’s article, how much more unmitigated was it in ‘Archaeology as anthropology’, Binford’s landmark paper written during an overnight boost of anger which shook the entire discipline. Seriously discontent with the culture-historical work of his teachers James Griffin and Robert Braidwood, the young Binford turned towards the neo-evolutionism advocated by Leslie White, the statistical methods of Albert Spaulding, and later also the positivist philosophy of science of Carl Hempel. ‘White was my philosophical model, and Spaulding was a methodological model, and Griffin became increasingly the challenge’ (Binford in Sabloff 1998: 13). In an interesting and amusing autobiographical reflection (though not entirely freed from self-glorifying justification), Binford (1972a) recalled how he grew dissatisfied with the criterions of individual competence, idiosyncratic intuition and personal acquaintance with the material which characterized the previous generation—the very criterions Thompson had been defending. Griffin was characterized as the prototype of the archaeologist who interpreted by intuition: ‘He loved the word “influence” [...]. Diffusion-and-the-plotting-of-cultural-influences-from-one-region-to-another became the name of the game’ (1972a: 3); Braidwood was said to be ‘the Sir Mortimer Wheeler of Rolling Prairie, Indiana’ (11). On the other hand, Binford appreciated Taylor’s attempt to do more with archaeology, but said that his method rested just as much on the ‘magic’ of interpretation as the work of Griffin and others had done: ‘Taylor had the aims but not the tools’ (1972a: 8). However, anthropologists like White, ‘the dragon slayer of Boasianism’ who taught Binford that Boas was ‘muddle-headed’, made their conclusions depend on sound reasoning rather than acquired authority: ‘His logic was made explicit, his interpretations were put out for criticism, and he supplied you with the criteria and rules for criticism’ (6, 7, 8).⁴ Yet cultural anthropologists taught Binford more than just reasoning. White’s definition of culture as ‘man’s extrasomatic means of adaptation’, Steward’s programme of a cultural ecology, Sahlins and Service’s critical reappraisal of nineteenth-century sociocultural evolutionism, American anthropologists in the late 1950s shed the shackles of the extreme historical particularism of the Boasian school and focused on cross-cultural comparisons, on

4 Some sections of Binford’s autobiography deserve further attention, especially where he likened the rise of the New Archaeology with the rise of Darwinism. If the traditional archaeologists were like ‘little Linnaean beings classifying things for the sake of classification’, White and Spaulding were ‘two Darwins [...] who together could provide some meaning for the endless taxonomic schemes of the archaeologists. White essentially refused to take archaeology seriously, and Spaulding appeared disinterested in theory. I was going to be the Huxley, the mouthpiece.’ Like Darwin’s bulldog, Binford used conferences and meetings as the preferred occasions for spreading the word; like his flamboyant Victorian predecessor, he was ‘angry, hurt, and intolerably arrogant’ (Binford 1972a: 9, 12).

adaptation, ecology and social evolution (Trigger 1989: 289-94).⁵ Not surprisingly, their works figured prominently in the bibliography of Binford's seminal paper.

Reading through 'Archaeology as anthropology', the differences with the older generation become immediately apparent on the level of literary style. The elegance and clarity of the humanist prose was now overshadowed by sentences on 'the demonstration of a constant articulation of variables within a system and the measurement of the concomitant variability among the variables within the system' (Binford 1962: 21). Although Binford's language was notoriously abstruse and obscure—'I never could write [...]. English was a mystery to me,' he once admitted (1972a: 6)—this change of literary style revealed a broader shift in the aims of the discipline. According to Binford, the objective of archaeology was no longer *imaginative reconstruction* of the past, but *logical explanation* of differences and similarities between archaeological complexes.⁶ He drew a sharp distinction between 'explication' (or reconstruction) and 'explanation' (or demonstration). Had individuals like Clark and Willey been calling for reconstruction beyond mere classification (the very ambition Smith had been so sceptical about), Binford now asked for rigorous explanation in terms of structures, systems, and processes. Along with Ascher he stated that 'archaeologists have not made major explanatory contributions to the field of anthropology' (1962: 21). And this is exactly what he attempted to alter, i.e. going beyond the 'theoretical vacuum' of the culture-historical approach: 'We cannot afford to keep our theoretical heads buried in the sand,' he wrote (21, 31-2).

Viewing cultures in terms of systems with integrated and interacting subsystems and regarding material culture as the extrasomatic means of adaptation, Binford presented a systemic view in which 'technology [was] closely related to the nature of the environment' (22). Although already at that stage he distinguished it from a mere 'environmental determinism' (22), it is clear that the previous notions on human uniqueness and nobility rapidly evaporated in such materialist and often mechanistic paradigm. Steward's cultural ecology taught that cultures were closely related to the potentials and constraints of the physical environment, even if they were not dictated by it.

5 Marvin Harris' cultural materialism continued to the neo-evolutionist renaissance in American cultural anthropology throughout the 1960s. Not surprisingly, he applauded ethnographic analogy and 'the extrapolation from contemporary primitives to paleolithic societies' (cf. 1968: 156).

6 In the New Archaeologists' prose logic became more important than literature. The past was no longer vivified through words but reconstructed through arguments. Imaginative writing, therefore, disappeared as a valuable asset to the archaeological enterprise. The New Archaeologists fell back upon a scientific, jargon-laden and often pretentious language (which was attacked by many of its opponents)—the writings of David Clarke and Lewis Binford being a case in point. In order to make archaeology more scientific, it helped to substitute the humanist literacy for the analytical writing style which prevailed in the natural sciences and positivist philosophy. Of all the New Archaeologists, Kent Flannery was perhaps the only one whose writings escaped the terse and dense prose of his fellow-innovators. Binford, however, maintained that 'the clearer writing is, the more ambiguous the terms are' (in Sabloff 1998: 63).

The most important consequence of this systemic framework was that it resulted in much more epistemological optimism. Indeed, instead of a historically unique and thus elusive entity, culture now became an explicable parameter within a systemic whole. If one believed that all cultural subsystems were integrated and in some way related to the structural properties of the physical environment, it became possible to extrapolate an understanding of one subsystem (like material culture) to other subsystems. In reply to Hawkes' and Smith's pessimism, Binford wrote:

It has often been suggested that we cannot dig up a social system or ideology. Granted we cannot excavate a kinship terminology or a philosophy, but we can and do excavate the material items which functioned together with these more behavioral elements within the appropriate cultural subsystems. (1962: 23)

And he added very forcefully that the structure of artifact assemblages 'should and do present a systematic and understandable picture of *the total extinct* cultural system' (23, original italics). In the archaeological record, there were 'technomic', 'sociotechnic' and 'ideotechnic' artefacts which were respectively related to the technological, social and ideological subsystem. The total cultural system was thus reflected in its material manifestations. Hawkes' ladder of inference lay flat on the ground.

'The past is knowable,' Binford once said (1968b: 99). The interpretative problem archaeologists ran into was not to be solved by empirical adequacy but by methodological ingenuity. Of course, good, well-preserved sites were an asset, but not a sufficient condition for understanding the past. It is telling that, throughout his career, which spans almost four decades, Binford never excavated a major site like Skara Brae or Star Carr. Unlike Childe and Clark, his name is not immediately associated with a particular dig. Of course, he worked on archaeological digs—first of native American settlements in Illinois, later of Palaeolithic sites in the French Dordogne—but the emphasis of his most important contributions was on theoretical problems and methodological issues, in particular statistics of lithic assemblages and later faunal analyses. The Mousterian debate and the dispute on early-hominid scavenging, to name but two examples, were predominantly based on reanalyses of excavated material rather than on personal digs.⁷

A new language (analytical), a new objective for archaeology (explanation), a new theoretical framework (systems theory, cultural ecology), a new epistemology (unmitigated optimism), and a new methodology (re-analysis), the paper implied

⁷ This, in fact, holds true for the other side of the 'Binclarke' tandem as well. David Clarke's work (1968; 1972b) was in the first place a theoretical reconsideration based on available material, not a study derived from personally excavated material. Archaeology was more than digging up the past; the 'undisciplined empirical discipline' did not proceed by empirical improvement alone.

a radical break with the previous generation.⁸ Surprisingly, somehow, it remained completely silent about the issue of ethnographic analogy. ‘Archaeology as anthropology’ was an attempt to bring the field of archaeology within the theoretical debates that had been going in anthropology, it was not an attempt to bring anthropology into archaeology. Since entire past societies were reflected in the material remains, there was no need to pull in external analogies. Binford’s epistemological optimism made the use of such ethnographic expedient redundant. In its earliest stage, then, the New Archaeology could do without ethnographic analogy. However, despite Binford’s silence on the theme, in an indirect way his paper helped to kindle a renewed archaeological interest for ethnography. The idea that material culture could reflect the total cultural system begged the question of how this actually happened in practice today. In the second half of the 1960s, a younger generation of archaeologists grew increasingly fascinated by how the correlation between social practice and material remains manifested itself in the contemporary world.

Fieldwork and cautionary tales

Already in 1939, Thomson was breaking new ground when he studied the remains of contemporary aboriginal settlements in Australia. It was reinvented in the 1950s by his near namesake Thompson who looked at modern Yucatan pottery making traditions (cf. Longacre 1978). Likewise, Crawford (1960) upon his return from Sudan argued that archaeologists had to do this sort of fieldwork themselves instead of reading the works of sociocultural anthropologists. And Ascher embraced the suggestion, made by Kleindienst and Watson (1956), that ‘the archaeologist turn to the living community to compile his own inventories’ (1961: 323). Well before the start of the New Archaeology, then, several archaeologists thought about or even undertook ethnographic fieldwork from an archaeological perspective. From the early 1960s onwards, however, this idea was put into practice by an increasing number of scholars. Ascher studied the Seri Indians in western Mexico (1962; 1968); Patty Jo Watson (1966) observed traditional rural life in Iran as part of a research project on early villages in the Near East; Karl Heider (1967) worked on the material remains of contemporary farmers in New Guinea; Richard Gould (1968), an ethnographer by training, turned to material aspects in his study of hunter-gatherers in Western Australia; Nicholas David (1971) studied settlement layout in Western Africa; Robson Bonnichsen (1972) did the same on

8 The question has often been raised to what extent the New Archaeology was really new (Binford 1968b; Clarke 1972b: 53–7). Whereas some writers have argued it represented a scientific revolution in the Kuhnian sense, others contended that all elements of the so-called new paradigm were there beforehand (cf. Sterud 1973; Meltzer 1979; Trigger 1989: 295). The insistence on ecology was already anticipated by the work of Clark, the system approach was borrowed from the New Geography and cultural ecology, the adaptationist view of culture came from neo-evolutionist anthropology, and the epistemological optimism echoed Taylor’s visionary programme for archaeology. Although most of the elements were there before the 1960s, a younger generation of archaeologists, under the leadership of Lewis Binford, integrated them into a coherent alternative which was as a whole quite innovative—although the fierce polemics of the 1960s and the self-perception of the New Archaeologists certainly exaggerated the break with the past.

an Indian site in the Canadian Rockies; and so on. Several suggestions were made as to the name of such ethnography for archaeological purposes. Watson called it ‘action archaeology’ (1966; cf. Kleindienst and Watson 1956); Gould preferred ‘living archaeology’ (1968) and Chang was presumably the first to use the term ‘ethnoarchaeology’ in a modern sense.⁹ Dealing with the question who should do the fieldwork he wrote:

Should the ethnologist observe and record these data so that they might someday be of some use to an archaeologist? Or should he do so in any event? Or should there perhaps be a branch of archaeology (ethnoarchaeology) to take care of such things? (Chang 1967: 230)

At the same time, other scholars were raiding the existing ethnographic literature to extract archaeologically relevant materials: Lewis Binford assembled ethnohistorical accounts of hide-smoking among North-American Indians (1967); James Hill checked descriptions of the architecture of contemporary Hopi and Zuñi Indians against excavated pueblos (1968); Kent Flannery scanned ethnographies for social parallels to his Oaxaca society in Mexico (1968); David Clarke looked at Bantu-Africa to substantiate his analytical categories (1968: 366-8); and Peter Ucko looked for descriptions of funerary practices on a world-wide scale (1969). The focus was largely on pastoral and agricultural societies; at this stage modern hunter-gatherers received only limited ethnoarchaeological attention (Gould being the only exception). It was only in the 1970s that foraging societies would constitute the main object of ethnoarchaeology.

Despite this increased interest in ethnography among a younger generation of archaeologists, there was no unified objective for the enterprise so that ethnography came to perform several functions, depending on the author. Yet all of these early ethnoarchaeological attempts, despite their diversity, had one thing in common: the role bestowed upon ethnography was rather minimal since archaeologists were at pains to circumvent the problem of analogy. Typically, publications from those years offer fairly detailed descriptions of the material aspects of pre-industrial life in the Near East, Africa, Australia or native America with a few hints in the introductory and concluding sections of the article on the broader archaeological relevance and on the need to pursue such work. Yet how the bridge from minute ethnography to archaeological conclusion was to be made, was generally left out.

Patty Jo Watson’s study of rural life in a small village community in northern Iran was already undertaken in 1959-60 as a part of Robert Braidwood’s Iranian Prehistoric Project. She detailed the land tenure system, the agricultural practices, the village architecture, the family composition, the material culture and argued that what was observed in modern Iran could be extrapolated towards ‘life in the earliest villages (approximately 7000-6500 B.C.)’ (1966: 19). Despite her innovative fieldwork, her theoretical framework came down to a straightforward direct-

9 The term ‘ethno-archaeologist’ had already been used in another context by the American ethnologist Hewkes at the turn of the century (Stiles 1977).

historical approach. In this sense, she continued the tradition of Willey and Clark but injected it with a new method, i.e. ‘action archaeology’.

Similarly, Hill’s functional interpretation of thirteenth-century architectural remains in Arizona through ethnographic analogy with contemporary Pueblo villages (1968), belonged to the long-standing tradition in Americanist archaeology of direct-historical reasoning, the only difference being that Hill was more systematic and explicit in his assessment. The same goes for Anderson’s interpretation of tools from Arizona through a Hopi analogy (1969) and Oswalt’s study of a Crow village in Alaska to explain nearby archaeological sites (Oswalt and VanStone 1967; Oswalt 1974). Oswalt very explicitly said that specific historical analogy was ‘the essence of ethnoarchaeology’; one had to work ‘with similar cultures living at essentially the same period of time and in the same locality’ (1974: 6).

Gould’s ‘living archaeology’ among the Ngatatjara of Western Australia had even more limited ambitions. Although he claimed to investigate ‘the “rules” of aboriginal site behavior’, comparable to ‘rules of grammar in a particular language’ (1968: 118, 101), in the end this resulted only in some general statements on Ngatatjara sites. The bulk of his early ethnoarchaeological work dealt with campsites, hunting blinds, wellsites, ceremonial sites, and painted caves; they were described with an amount of detail which was rare for an ethnologist, but the attempt to shed light on some aspect of prehistory, indigenous or not, was bracketed.¹⁰ In the conclusion, the Ngatatjara were said to ‘present new possibilities for archaeological interpretation’ but how this was to be done was not specified. Likewise, Stanislawski (e.g. 1974) observed contemporary Hopi traditions of pottery making and described them with admirable precision, but the feedback to specific archaeological problems remained at bay. At this early stage of ethnoarchaeology, pure documentation of rapidly vanishing traditions was regarded as a legitimate objective *per se*. Such preliminary description, it was believed, could prod the archaeological imagination and alleviate the ethnocentric bias.

In a very similar sense, ethnographic literature was thought ‘to widen the horizons of the interpreter’, as Peter Ucko claimed in his article on funerary remains (1969: 262). Ucko had scanned the available ethnographic literature on mortuary practices not to find ‘a one-to-one correlation between the acts of tribe A and the remains of culture B’ but in order ‘to suggest the sorts of possible procedures which may result in the traits characterizing culture B’ (263). Just like Crawford and Hawkes had intimated before him, ethnographic analogy was there to ‘stimulate your imagination’. If there was continued cultural tradition, it had to be welcomed. But if there was none, the comparison depended on ‘closeness of the fit’, that is, the amount of similarity (263). From his survey, for instance, Ucko learnt that the richness of a grave did not necessarily imply that one had to do with a wealthy individual.

10 In later work, however, Gould has tried to incorporate his ethnographic observations into his archaeological interpretations of the same region (e.g. 1974). Yet even then he remained within the confines of a direct-historical analogy, though the time range he covered consisted of several thousands of years. Continuous models, as he preferred to call them, had ‘an inherently higher degree of probability’ (1974: 39).

Flannery's (1968) use of ethnography followed the more traditional line of looking in the present for an entity which paralleled the excavated past. Having defined that prehistoric Oaxaca was a developing society where individuals used imported and exotic raw materials to enhance social status, he sought ethnographic similes to buttress this conclusion. The Tlingit and the fur trade and the jade trade in highland Burma were invoked to substantiate this claim. Even if the choice was rather gratuitous, ethnography was thought to corroborate archaeological conclusions.

Ethnography could also undermine certain archaeological dogmas. David Clarke (1968: 365–88) used ethnographic literature on the Bantu to show that there was no total correlation between language family, ethnopolitical group, physical race and material culture. Here, ethnography served to debunk the tenets of culture-historical archaeology (in particular the equation that 'archaeological cultures are ethnic groups'), much along the same lines as Tallgren had done with his Siberian case-study in the 1930s. According to Clarke's 'polythetic culture concept', there were no neat boundaries between distinct cultures, only affinity levels.

Likewise, White and Thomas (1972) showed that much of the formal types archaeologists worked with had no ethnic relevance whatsoever. Participating in the long-standing debate as to whether archaeological taxa should reflect indigenous mental templates (emic or etic taxonomy), their study in the highlands of New Guinea showed that native stone-knappers drew quite different taxonomic distinctions than the archaeologists had done. They used this emic taxonomy to interpret prehistoric finds from the same region and suggested it might have a wider application.

As time went on, this critical function of ethnography became more and more prominent in archaeological debate and the journal *World Archaeology*, which started to appear in 1969 under the editorship of Peter Ucko, became an important locus for the ventilation of such criticisms.¹¹ For many archaeologists in the late 1960s–early 1970s, the living present became not simply the touchstone for our ideas about the remote past; it was first of all the place where our 'methodological naiveté' (Binford 1968b: 96) and 'archaeological innocence' (Clarke 1973) became blatant. The most popular use of ethnoarchaeology consisted of providing the archaeologist with cautionary tales. No matter whether one reads Heider's analysis of the Dugum Dani in New Guinea (1967), Cranstone's paper on the 'Neolithic' Tifalmin in the same country (1971), David's interpretation of the Fulani compound in Cameroon (1971), or Bonnichsen's study of Millie's camp in Canada (1972), the message was essentially the same, i.e. that archaeologists are poorly equipped to recover and interpret what has actually happened on a specific site. In this sense, ethnoarchaeological cautionary tales reverberated the interpretative pessimism of British prehistorians in the 1950s. David invited the reader

¹¹ In 1971, *World Archaeology* had a theme issue on ethnography for archaeologists. Of the five papers, three were confined to the safe direct-historical approach (Gould 1971; Van der Merwe and Scully 1971; Lauer 1971) and two were straightforward cautionary tales (David 1971; Cranstone 1971), joint by a similar article one year later (Bonnichsen 1972).

to interpret the ‘archaeological’ plan of a Fulani village in terms of household reconstruction. At the end he gave the true, ethnographic version of the site and its occupants, his paper being meant to ‘illustrate the errors that may arise’ and to ‘indicate some likely limits of possible reconstruction’ (1971: 125). Likewise, Bonnichsen interpreted a recently-abandoned Indian campsite and later checked his conclusions with those of ‘Millie’, one of the former occupants, only to find he had committed ‘a sequence of errors’: items had been misidentified, false associations had been made, activity areas had been interpreted incorrectly, and relationships between activity areas had been misinterpreted (1972: 286). And Heider (1967: 63) finally concluded: ‘Reconstruction of prehistoric behavior is not by any means impossible, but it is terribly difficult. [...] If this Cautionary Tale from New Guinea has a moral for the archaeologist, it is, “Don’t simplify.” ’

In the second half of the 1960s, the function of ethnography for archaeology served as a basis for the direct-historical approach (Watson, Hill, Anderson), as a means for widening the horizon (Gould, Stanislawski, Ucko), as a confirmation of archaeological ideas (Flannery), as a refutation of archaeological ideas (Clarke, White and Thomas), and, most importantly as a cautionary tale for archaeological interpretation (Heider, David, Bonnichsen). Despite the variety, these were all rather minimal options. At no point was it convincingly demonstrated that ethnography could make a constructive contribution to specific archaeological interpretations beyond the cases of demonstrable historical continuity. Ascher’s call for ‘a new analogy’ notwithstanding, such objective was far from being rapidly realized. This holds even true for Ascher’s own case-studies with the Seri Indians (1962; 1968) which made not clear how ethnographic information could be successfully applied to archaeology. Put briefly, early ethnoarchaeology was critical rather than constructive; it taught the Socratic lesson that archaeologists only knew that they didn’t know. Though scholars were fascinated to study the link between social practice and material remains, they found out that there was no single, causal mechanism between the two, let alone a straightforward means of extrapolating it towards the past. In the highlands of New Guinea, at the campsites in the Rockies, and in ethnographic libraries stacked with descriptions on mortuary practices, one thing became clear: ‘Don’t simplify.’

Hypothetico-deductive reasoning or the benefits of testing

If in the context of the emergent New Archaeology the enthusiasm for ethnographic fieldwork was ubiquitous, the avidity for ethnographic analogy was much more disputed. To understand the minimal role of ethnoarchaeology as a warning finger requires a closer look at the analogy debate of the 1960s. The tension between ideas put forward by Chang and the ensuing critical reaction of Binford are especially revealing.

In a paper published in *Current Anthropology* in 1967, the Chinese-American scholar K.C. Chang drew attention to the ‘major aspects of the interrelationship of archaeology and ethnology’, as his title read. Predictably, one of these concerned the issue of analogy. Chang held that ‘analogy is the principal theoretical apparatus by which an archaeologist benefits from ethnological knowledge’ (1967: 229).

In his view, analogy was the ‘cornerstone’ of archaeological reconstruction. Following the ideas of Thompson and Ascher who had accorded a central role to analogy in archaeological inference, he argued that ‘in a broad sense, archaeological reconstruction *is* analogy’ (230, original italics). Chang applauded Ascher’s call for a new ethnographic analogy and wrote: ‘No archaeologist is worth his salt, it can almost be said, unless he makes an analogy or two in every monograph he writes’ (229).

As the paper was published as a discussion article in *Current Anthropology*, it was obvious that Binford who had, after all, written on the relationship of archaeology and anthropology, would say a few words. Binford disagreed with Chang on nearly all levels. The aim of archaeology was not reconstruction, he repeated, but explanation of cultural differences and similarities. Analogy was not the best device to make such interpretation, but a highly contentious form of reasoning: ‘Even if we were to admit that archaeological arguments always include an analogical component (and I am not convinced of this), this does not make these arguments analogies’ (Binford in Chang 1976: 235). Sure, an ethnographically known context might suggest a behavioural correlate for an archaeological problem but ‘recourse to ethnography could never render such an argument probable or true’ (236). Unlike Thompson, Ascher and Chang, Binford did not believe that the solution for archaeological inferences was to be sought in the ethnographic field itself. His position echoed that of Childe and G. Clark: analogy provided useful questions but no definite answers; eventually, the archaeological record had to have the final word. An analogical proposition was all well and good, it was only through testing against the archaeological evidence that its value was ascertained: ‘The basic form of archaeological argument [...] should be logico-deductive. From a set of premises, we can frame testable hypotheses whose confirmation will lend support to the postulates and assumptions (premises) on which the hypotheses are based’ (235), he wrote in response to Chang.

The importance Binford attached to this aspect of testing cannot be overestimated. As a consequence of his readings in Hempel’s positivist philosophy of science, he embraced the idea of a hypothetico-deductive logic whereby scientific evidence leads to hypotheses, hypotheses to testable deductions, deductions to tests, and tests to confirmations or refutations of the initial hypotheses. Collingwood’s hermeneutic which filtered through in the historically inspired archaeology of the 1950s had to make place for Hempel’s positivism. Empathic ideas were meaningless, unless they were logically validated (cf. Binford 1983a: 21). This set of arguments came up in all of Binford’s publications published in the late 1960s (Binford 1967; 1968a; 1968b). At the Man the Hunter conference, he said: ‘It is only through the testing of hypotheses logically related to a series of theoretical propositions that we can increase or decrease the explanatory value of our propositions’ (1968a: 269). In the introduction of the influential volume *New Perspectives in Archaeology* he demanded for ‘rigorous testing of deductively drawn hypotheses against independent sets of data’ (1968b: 86).

As in all positivist philosophies of science, a sharp distinction was drawn between the context of discovery and the context of justification. Hypothesis formulation, where analogy played its role, belonged to the first of these; hypothesis testing to the second. Whereas the context of discovery entailed the fortuitous and accidental process of scientific practice (a domain more important to the historian of science than to the practising scientist *per se*), it was only in the context of justification that science gained its logical credibility through rigorous testing. Analogy thus belonged to the less important stage of hypothesis formulation, a place where it could not affect the sound procedures of hypothesis verification. Binford said:

The crucial point, however, is that our understanding of the past is not simply a matter of interpreting the archeological record by analogy to living societies as has been commonly asserted (cf. Thompson 1956, p. 329). Our knowledge is sound to the degree that we can verify our postulates scientifically, regardless of the source of their inspiration. (Binford 1968a: 269)

‘Regardless of the source of their inspiration’: even if Binford applauded the recent growth in ethnoarchaeological research, he maintained that ‘refinements in data collection and increased ethnographic knowledge cannot by themselves increase our knowledge of the past’ (1968b: 86). This explains why Binford himself, at least at this point of his intellectual development, did not start to undertake ethnographic fieldwork like so many others. Analogy, imagination, conjecture, and empathy were acknowledged as interesting devices for gaining new ideas, yet ‘a consciously deductive philosophy’ went further and undertook ‘the verification of propositions through hypothesis testing’ (1968b: 90-1). As a consequence, Binford concluded: ‘the archaeologist is in no way dependent upon the ethnologist’ (1968a: 269).

The most elaborate example of Binford’s alternative reasoning was to be found in his famous article on ‘Smudge pits and hide smoking’ (1967) where he tried to interpret the presence of certain features he had excavated in Illinois; they concerned small pits filled with charred corncobs at the edge of native American settlements. In essence, Binford invoked analogical reasoning for the solution of a classical problem, i.e. the functional interpretation of an enigmatic find category. In this, he did not differ from seventeenth-century antiquarians who used ethnology to explain the so-called thunderbolts as tools. He did, however, differ from them in his insistence that the ethnographic suggestion should be thoroughly tested against the archaeological record. Based on a survey of ethnohistorical sources, he postulated that the smudge pits must have been used during the smoking of animal hides. He then proceeded to derive a number of testable hypotheses from this postulate. And although he failed to perform the actual tests themselves (a painful omission which became apparent later), he concluded that the procedure discussed was ‘appropriate in the context of a positivistic philosophy of anthropology and archaeology’ as ‘the final judgement of the archaeological reconstruction presented here must rest with testing through subsidiary hypotheses drawn deductively’ (1967: 48-9).

The Chang-Binford debate was not an isolated dispute between two North-American archaeologists but was framed by the much larger question in anthropology as to whether modern hunter-gatherers could be used as models for pre-history. This entailed nothing less than a renaissance of the nineteenth-century debate on the position of contemporary savagery. The neo-evolutionist Elman Service contented that there was much to be gained from a study of 'our contemporary ancestors' (1962: 8). Even if he admitted that 'primitive' societies were affected by contact with the industrialized world, he still could write that 'anthropology possesses a time machine' (8). For Leslie Freeman (1968) this could not be permitted: societies of modern hunter-gatherers were too much the result of complex historical processes. Repeating the argument made by the Duke of Argyll a century earlier, he noticed that the spread of agriculture had forced historical and contemporary foragers into marginal zones so that they were 'totally unrepresentative of the sorts of hunting-gathering adaptations that existed before the advent of food production' (1968: 264). He further stressed that there were 'logical limits to prediction from a limited sample': not all of human prehistory could be deduced from the few remaining forager societies today. Amount of immediate similarity, therefore, was no criterion to justify the use of analogy:

The use of assumed similarities with modern behavior in the explanation of the behavior of extinct groups is not only fallacious, it is also deleterious to research since it prevents the discovery that the postulated similarities do not exist.
(Freeman 1968: 265)

Whereas Freeman eschewed the use of any form of analogy, others tried to limit its impact by calling, like Binford had suggested, for independent testing of the analogical propositions against the archaeological record. For example, Hill (1968) interpreted a thirteenth-century pueblo in Arizona by analogy with contemporary Hopi and Zuñi architecture but sought to validate his propositions through systematic archaeological confirmation. His approach was soon staged as an exemplary case of hypothetico-deductive reasoning so typical for the New Archaeology. In their influential volume *Explanation in Archaeology: An Explicitly Scientific Approach*, Watson, LeBlanc and Redman applauded it by rejecting the taken-for-granted distinction between a direct-historical and a general-comparative analogy: 'The logical framework for application of both kinds of analogy remains exactly the same: regardless of their source [...] the proposed analogies are simply hypotheses. As such they must be tested against independent archaeological data' (Watson, LeBlanc and Redman 1971: 50-1). In contrast to Clark and Willey's opinion, the direct-historical approach was thus no longer the superior form of analogy. Elsewhere, Watson stated: 'Logically speaking, it does not matter where these interpretative hypotheses come from; what matters is how they stand up when tested against the archaeological record' (1979a: 277). David Clarke, too, endorsed this opinion. In *Analytical Archaeology* the role of ethnographic analogy was limited to critical examination of theory; yet in *Models in Prehistory* he spent considerable attention to 'the morass of debate about the proper and improper use of historical and ethnographic "parallels" in archaeological interpretation' (1972b: 40).

Regarding ethnographic analogies as a specific form of model-building, he said that they may ‘generate potential models’ but that ‘only archaeology can test their applicability in a given archaeological context’ (41). Like Binford, he did not bother whether the hypotheses were ‘imagined, deduced or dreamed by the archaeologist’, whether they came from ‘historic-ethnographic analogy’ or ‘from movements of molecules in a gas, the mutual adjustments of magnets floating in a bath, or the boundary patterns of bubbles in a soap film’; the only important aspect was that they ‘be tested against the archaeological record’ (41, 42).

Whereas Ascher and Ucko had still considered ‘closeness of fit’ (i.e. the amount of formal similarity) as the decisive yardstick for the analogy’s success, the testing programme inaugurated by Binford, Watson and Clarke enabled to reduce the amount of similarity to a secondary place in the evaluation. ‘Goodness of fit is not enough,’ David Clarke exclaimed (1972b: 41). Source and target did no longer need to be as close as possible; a certain distance was permitted now that testing independently verified, validated or refuted the analogical hypothesis. Consequently, the source analogue was not necessarily required to show historical continuity with the archaeological target (as Watson argued), it could even come from remote fields like the physical sciences (as Clarke argued). If only rigorous testing was performed, almost anything could be a potential source of inspiration, the New Archaeologists optimistically reasoned.

Between critique and inspiration

In the 1960s, a young generation of American archaeologists sought to align themselves much more closely with anthropology. In articles with titles like ‘Archaeology as anthropology’ and ‘Major aspects of the interrelationship of archaeology and ethnology’, they strove to benefit from neo-evolutionist and adaptationist ideas then en vogue in anthropology. Simultaneously, several archaeologists started to explore the ethnographic field to extract data relevant to a set of inquiries uncovered by traditional ethnographers. Ethnoarchaeology was born and the link between cultural behaviour and material residue became its prime focus of attention. Inevitably, this led to a discussion on the role of ethnographic analogy in archaeological inference. Whereas most scholars believed that much was to be learned from a more intimate understanding of processes occurring in the present, none really used evidence on contemporary non-industrialized societies as a basis for archaeological inference beyond the confines of the traditional direct-historical approach. The role of analogy was limited to a critical function (most notably in the format of cautionary tales) or to an inspirational function (in the formulation of hypotheses). While the former suggested how little we actually knew, the latter indicated how much we could possibly know. Yet despite this diversity, both strategies were fundamentally similar in that they sought to circumvent the thorny issue of analogy. Authors who presented a cautionary tale avoided the use of analogy and withdrew from making any definite or constructive statements about the past. Authors who relegated analogy to the realm of mere inspiration, eliminated it from the context of verification (Wylie 1985: 85-8). Analogy thus belonged to the more trivial context of discovery. As hypothesis-testing was thought to be the

decisive moment in archaeological reasoning, it did not even matter where the hypothesis initially came from. The danger of induction which any analogy implies, was thus eliminated by a trust in hypothetico-deductive logic.

The consequence of such logic is that analogies are not assessed on the basis of their internal validity but on the truth of their conclusions. Put simply, if you test an analogy, you don't care to improve its structure—you're only interested in its content. Although a number of archaeologists started to draw upon the work on analogy by contemporary logicians, the latters' procedures for strengthening the validity of the analogies were often mentioned but neglected at the same time. Binford, for instance, drew upon the work of the logician Stebbing and referred to the criteria of 'relevance' and 'weight of the conclusion' (although he used different names), yet he continued:

In the examination of anthropological arguments from analogy, we are not concerned with the criteria which will allow us to judge the form of a particular argument from analogy as in the previous discussion [on Stebbing]. Instead, we are concerned with the content of the argument. (Binford 1967: 35, original emphases)

By the same token, Nicolas Peterson (1971) enumerated in an edited volume on aboriginal Australia Copi's six criteria for assessing the strength of an analogical argument. It was one of the first elaborate references to the logic of analogy proper but, like Binford, he refrained from putting these suggestions into practice. Clarke, too, had an incipient interest in the logic of analogies when he said that the book on 'the valid procedures for using these models' was 'yet to be written' (1972b: 40) but finally contended with testing as the appropriate solution.

The New Archaeologists were aware of the problems of analogical reasoning but had not yet found a solution to outstrip the single-minded reliance on similarity of the Victorian evolutionists and mid-century humanist archaeologists. In the absence of such improved logic, critical warning and external testing were the two tentative way outs. Though certainly overstating the point, Bryony Orme's words from 1973 contain a grain of truth for the period she wrote in: 'I would even claim that, allowing for cultural differences, the use made of ethnography has not changed since the early modern period, let alone since the heyday of the 19th century' (1973: 487). The next decade, however, would bring about fundamental changes—particularly because the scholar who had been the staunchest defender of analogy-testing started to doubt.

The heyday of ethnoarchaeology

Throughout the 1970s and well into 80s, ethnoarchaeology thrived as it had never before. What had once been a relatively minor branch of the New Archaeology restricted to critical and inspirational purposes soon developed into one of the

largest fields of ‘processual archaeology’ (as the New Archaeology increasingly started to be called after it was no longer new). Many North-American archaeologists began to undertake ethnographic fieldwork, not only in the planet’s remotest areas where the last hunter-gatherers tried to eke a living out of their marginal ecological conditions, but also closer at hand in certain segments of the Western world like supermarkets, camping grounds and car graveyards. More than a dozen monographs and edited volumes appeared; archaeological and anthropological journals incessantly published articles on contemporary living contexts; and generations of undergraduates were exposed to the latest insights on the theme. But more than this quantitative growth (which could be explained by the remarkable increase in professional archaeologists and students after the 1960s), there was also substantial qualitative change. Theoretical issues were high on the agenda; the discussion on analogy continued with arguments which had never been brought up before; and logic was drawn in to improve the format of reasoning. Whereas some other fields of processual thought, like the debate on style, constantly run into conceptual boundaries, the discussion on analogy was refined over the course of nearly fifteen years. This is not to say that there was unambiguous progress or that perfection was reached; only that the use of ethnography in archaeology had moved into a rich, complex, and multifarious stage of its history.

What must be held responsible for this popularity? It would be fallacious to uncritically link the successes of ethnoarchaeology to the work undertaken in the 1960s. Although someone like Gould simply continued and expanded the research he had done earlier, for Binford there were fundamental problems with the testing dogma as it had gained widespread acceptance, partly under his instigation. To understand ethnoarchaeology in the 1970s requires first to understand Binford’s change of opinion.

The impossibility of independent testing

Two years after Binford’s smudge pits article, *American Antiquity* published a contribution by Patrick Munson (1969) which questioned the validity of Binford’s interpretation of these features as hide-smoking devices. From a number of ethnohistorical documents, Munson suggested ‘also on the basis of analogy’ (83) that the smudge pits might be better interpreted in the context of smoking ceramics: pottery would thus have been put over the pits in order to have the interior blackened. Binford wrote a reply but *American Antiquity* refused to print it as the editor thought it did not add to the discussion. The paper eventually appeared in Binford’s first collection of articles (Binford 1972b) and attentive reading shows that it was far more than a stubborn attempt to be proven right.¹² Even if Binford rebutted Munson’s critique by showing that ethnohistorical sources on pottery-smoking were inadequate—the best example dated from a postcontact moment whereby pottery was smoked inside a European style house—he did not entirely

12 *American Antiquity*’s refusal to publish the reply made Binford drop his subscription to the journal and thus his membership of the Society of American Archaeologists, the prime professional organization of archaeologists in North-America, for almost a decade (Binford 1983b: 19)

reject the alternative: ‘The accuracy of a proposition can only be determined by the testing of deductively drawn hypotheses against independent data, something which has not been done thus far for either proposition’ (1972b: 56). Hide-smoking or pottery-smoking, Binford thus admitted that ‘neither proposition has been tested’ (53). The adequate testing of a proposition seemed far more difficult than originally acknowledged. Prior to such testing, however, Binford suggested that ‘the relative merit of different arguments from analogy’ be assessed ‘on the basis of the internal logic and strength of competing arguments’ (53). Instead of external verification of an analogical proposition, it was worthwhile to check in the meantime its internal validity because ‘an argument may be logically valid but at the same time false’ (52). He regarded Munson’s alternative as logically valid, but reasoned on the basis of available ethnohistoric evidence that it was empirically less likely. His conclusion is worth quoting in full:

The argument presented here is that the presence of a valid alternative proposition based on ethnographic analogy is not sufficient to refute an equally valid but different proposition. A valid proposition can only be refuted through hypothesis testing. However, when faced with valid alternatives, one can evaluate in probabilistic terms the relative strength of alternatives and make decisions as to how to invest research time in hypothesis testing. (Binford 1972b: 57)

Although Binford still supported independent testing as the ultimate criterion in hypothesis-evaluation, he never undertook such verification himself, even when confronted with a radical alternative. This reluctance was telling of the intrinsic difficulties testing implied: the archaeological record seemed a poor arbiter to decide between two available options. In fact, Binford had to rely on ethnohistoric rather than archaeological evidence to evaluate Munson’s proposition. Whereas Munson’s article could have been easily eliminated on the basis of these sources, to Binford it ‘pointed to even further problems with the use of arguments of analogy’ (1983b: 19). Even if he still regarded the hide-smoking interpretation as ‘one of the strongest arguments from analogy I have ever seen in archaeology’ (1983b: 8), in retrospect, he said he was ‘fascinated’ by Munson’s alternative: ‘His work appeared to me to be just the type of constructive exploration into our problems of inference that would move us toward a better understanding of our methods’ (1983b: 19). It had revealed two important insights: *primo*, the difficulty of testing the analogy’s empirical content, *secundo*, the necessity of strengthening the internal logic.

Similar doubts about the value of independent testing had emerged within the context of another and much more famous debate Binford was involved with in the late 1960s, i.e. the Mousterian debate. In opposition to François Bordes, the dean of European Palaeolithic archaeology who regarded the distinct Mousterian facies in south-west France as representing different ethnic entities, Binford argued that they reflected different sorts of functional activities. The Mousterian debate was hailed as the first triumph of American processual archaeology over European culture-historical archaeology, but Binford himself became far less convinced of any successes booked. The Mousterian debate, he recalled, was not so

much a step forward in European Palaeolithic archaeology but ‘the key test case for learning about the limitations of our methodologies’ (1983b: 66). Armed with the newest statistical techniques and a pile of punch cards, he and his wife Sally Binford had set out to the Dordogne in 1968 to study the assemblage variation and faunal associations of Mousterian levels from Combe Grenal. Yet what had come up was ‘an absolute profusion of new facts’ (1983b: 66) as well as a sense of ‘total frustration’ (in Sabloff 1998: 18). Binford has extensively written about this phase of his intellectual career and while there is a danger of historical distortion in using such retrospective statements, his own posterior reflections are often more illuminating and honest than the brash polemics he wrote in the 1960s. ‘True to the beliefs of the times we thought we could test our ideas against the archaeological record at the Mousterian sites,’ he wrote in 1983, but despite all the new data he rapidly came to the conclusion that ‘we had no Rosetta stone, no way of translating the wealth of patterns into meaningful statements about past conditions [...]. The important step of “testing” had not been performed [...] our interpretative principles were inadequate’ (1983b: 66, 67).

Binford’s student James Hill, too, had tried to test the ethnographic predictions about the function of rooms in the famous Broken K Pueblo in Arizona but started to realize that such ‘independent tests’ actually came down to standard archaeological arguments for functional interpretation. ‘What neither of us faced squarely at the time,’ Binford (1983b: 12) wrote, ‘was that we could not use the archaeological record to test the accuracy of meanings assigned to archaeological facts by these tactics.’ Throughout the 1960s, Binford and his students had assumed that the archaeological record could serve as the independent arbiter for inferential propositions. Now he started to realize that this was not the case, that the archaeological record consisted of mute statics, that it was contemporary, that our observations on the past were observations in the present, and that the idea of independent testing suffered from a latent incongruity: ‘If one is testing an interpretative principle linking static residues to causal or conditioning dynamics, one cannot test such a hypothesis against archaeological data where only static material exists empirically’ (Binford 1983b: 14). In a more succinct version, this contradiction became: ‘one cannot readily test an argument relating statics to dynamics when all one has are static facts’ (15). All so-called independent tests remained therefore ‘intuitive, impressionistic, and unevaluated’ (17).

The very moment Binford realized that ‘the problem of testing seemed particularly sticky for archaeologists’ (1983b: 12), a second-generation of New Archaeologists came up with a form of positivist logic which was much more extreme than anything that came before. Authors like Fritz and Plog (1970) and Watson, LeBlanc and Redman (1971) urged that the hypothetico-deductive form of reasoning be expanded to a strict deductive-nomological approach: hypothesis-testing was not enough, it had to entail law-building as well. Borrowing the covering-law model of explanation from positivist philosophers like Nagel, Hempel and Oppenheim, they insisted that hypothesis-testing in archaeological explanation required the formulation, confirmation and utilization of general laws. A particular archaeological phenomenon was only duly accounted for if and only if

it could be subsumed under a general, covering law; if this was not the case, the interpretation had to be rejected as unscientific. Testing thus became an integral part of a broader, ‘explicitly scientific’, nomological approach: ‘One distinctive feature of scientific archeology is a self-conscious concern with the formulation and testing of hypothetical general laws,’ was the call to arms with which the book by Watson c.s. opened (1971: 3). However, many contemporaries found that there were severe problems with this programme. One year after its publication in an important but somehow forgotten paper published in *World Archaeology* (1972), the logician Morgan mercilessly exposed its selective borrowings, logical inconsistencies, rhetorical verbiage and outright sloganeering. Most importantly, he said that the examples cited did not confirm to the covering-law model but were in fact nothing more than disguised inductive-statistical arguments. Binford, too, regarded this ‘overemphasis on method or logic’ as ‘a return to an inductive strategy’ (1977: 6). According to him, there was nothing wrong with empirical generalizations but these did not result in *general* laws; they were simply inductively arrived at *empirical* laws and as such insufficient for theory-building (Binford 1978a). Despite Binford’s initial positivism, the notion of ‘law’ had always been of minor importance to him: ‘I do not believe laws of relevance to explanation can be discovered and evaluated through inductive arguments, nor do I presume that confirmation is “contained within the framework of the covering-law model of explanation”,’ he wrote in response to some of the hyperpositivist proponents of the deductive-nomological approach (1978a: 42). He, therefore, found the work of Fritz and Plog ‘most misleading’, and that of Watson, LeBlanc and Redman ‘very frustrating’, in particular because these were increasingly cited as the epistemological foundations of processual archaeology (1983b: 14, 15).

In the wake of Binford’s criticism, several scholars accepted the impossibility of independent testing. !Kung ethnoarchaeologist John Yellen agreed that ‘deductive proof—in the strictest sense of the term—is difficult to obtain’; he thus denounced the ‘explicitly *scientific approach* to archaeology’ with its ‘unfounded optimism and undeniable arrogance’ (1977: 2, 12, original italics). Daniel Stiles admitted that in practice an archaeological ‘test’—the word was always put between inverted commas in his writing—simply came down to ‘the degree of similarity’ and ‘the goodness of fit’ between source and target analogues (1977: 96, 97). Testing thus fell back upon the very inductive criterion it had tried to escape, that is, that of enumerating similarity. Even an ‘explicitly scientific archaeologist’ like Patty Jo Watson had to make a compromise: ‘*logically* it does not matter [where the analogy came from], but—as is so often the case—*practically* it does matter’; the most suitable sources were still to be found in ‘settings as much like the prehistoric ones as possible’, preferably there where ‘cultural continuity is great’ (1979a: 277, 278). Grahame Clark must have nodded approvingly. Similarity slipped in by the back door: whereas Watson had once rejected the distinction between the direct-historical and the general-comparative approach, the former was now admitted to be the more productive one as it showed greater resemblance between source and target. Indeed, she wrote that the immersion in an ethnographic situation that paralleled the prehistoric context could be ‘so overwhelming

that at times it verges on a mystical experience' (1979a: 278)—curious language for someone who had proclaimed an explicitly scientific archaeology.

From the 1980s onwards, the pessimism on testing turned into a full-blown critique. Hodder (1982a: 21), for instance, argued that the archaeological confirmation of test implications consisted of nothing more than simply adding further similarities to the ones which had already been established at the beginning of the analogy. Wylie (1985) demonstrated that the hypothetico-deductive method of testing propositions could never escape a dependence on inductive arguments. Likewise, Stoczkowski (1992) showed that for the formulation of useful test implications, archaeologists had to make a second recourse to analogy. It was impossible to directly confront the testable hypothesis with the archaeological record without relying on inductive assumptions about the relationship between statics and dynamics. The criterion of independent testing was thus shown to be a myth born out of an exaggerated positivist avidity.

'We cannot prove anything positively,' Binford came to believe (1977: 3). Rather, the major challenge which archaeologists faced was 'not testing laws [...] but instead justifying the linkage of a causal dynamic [...] to the facts used in reconstructing the past' (1983b: 16). According to him, the only way out of the impasse created by the impossibility of testing was to 'study situations where organized dynamics were taking place and where patterned statics were a natural byproduct of the dynamics' (1983b: 68). This was Binford's somewhat convoluted call to do ethnoarchaeology, i.e. to study the causal relationships between cultural behaviour and material residue, to develop a middle-range theory which would like a Rosetta stone 'permit the accurate conversion from observation on statics to statement about dynamics' (1981: 25). Half a decade later than many of the other New Archaeologists, Binford was convinced that much could be gained from a study of contemporary non-industrial societies. Not so much because this enhanced our understanding of the past, but because it enhanced the understanding of our understanding of the past. It was quite literally an exercise in 'learning how to learn' (Binford 1983b). From 1969 to 1973, Binford spent several seasons with the Nunamiut in northern central Alaska, one of the last predominantly hunting societies in the world; it was followed by a shorter stay with the Alyawara, a tribe of Australian aborigines, in 1974. If the New Archaeology was to be more than 'an antitraditional archaeology at best', more than 'an anarchy of uncertainty, optimism, and products of extremely variable quality', students of the past had to seriously investigate how inferences were drawn in the present (Binford 1977: 9). The rebellion could not continue 'for rebellion's sake'; what was to be done next was 'the difficult task of theory building and methodological development' (9, 10). The result of these important fieldwork studies appeared during the late 1970s and will be discussed further on.

Yet the idea of studying the present as a place where statics and dynamics could be simultaneously observed provided the justification for most, if not all, ethnoarchaeology at the time. Yellen went so far as to speak about a 'laboratory approach' (1977: 11) where direct observation of an ongoing society could be correlated to its material by-products. Kramer wrote that the task of ethnoarchaeology

was ‘to systematically define the relationships between behavior and material culture’ (1979: 1); Stiles focused on ‘the imprint that this behaviour will leave on the physical world’ (1977: 91); and Longacre sought to discern ‘material correlates of patterns of human organization and behavior’ (1978: 362). Ethnoarchaeology was no longer a quest for parallels, but a study of processes.

A thriving subdiscipline

In the second half of the 1970s, many university libraries must have spent fortunes in keeping up with all the publications which explored the interface between archaeology and ethnography. The period between 1974 and 1982 saw the appearance of a series of books with titles like *Ethnoarchaeology* (Donnan and Clewlow 1974), *Archaeology and Anthropology: Areas of Mutual Interest* (Spriggs 1977), *Archaeological Approaches to the Present: Models for Reconstructing the Past* (Yellen 1977), *Explorations in Ethnoarchaeology* (Gould 1978a), *Living Archaeology* (Gould 1980), *Ethnoarchaeology: Implications of Ethnography for Archaeology* (Kramer 1979), *Anthropology for Archaeologists* (Orme 1981), *Ethnography by Archaeologists* (Tooker 1982) and *The Present Past: An Introduction to Anthropology for Archaeologists* (Hodder 1982a). Even if this list of variations on a theme is far from being exhaustive (it leaves out essential publications which appeared as articles), it gives at least an impression of ethnoarchaeology’s popularity at the time. Ethnoarchaeology, it seems, was not just a finicky debate conducted in scientific journals, but a profitable branch of social science for publishers.¹³ A questionnaire among practising North-American archaeologists in 1977 revealed that from the 137 current methodological and theoretical research frontiers and issues mentioned by the respondents, ‘ethnoarchaeology’ ranked third in importance (on the first and second place stood ‘the need to develop better models for understanding the processes of cultural evolution’ and ‘the use and abuse of quantitative methods’; Schiffer 1977). In the same year, Stiles’ state of the art in ethnoarchaeology optimistically claimed that the field had overcome ‘many of its growing pains’ (1977: 87). In 1982, the first issue of the *Journal of Anthropological Archaeology* appeared with Bob Whallon as editor, one of Binford’s first students. True to its title, the archaeological and anthropological study of ‘contemporary, living human groups’ was listed as one of the main targets in the editorial introduction (Whallon 1982: 2). The very first paper was a contribution by Binford on the organization of place among the Nunamiat.

Some general lines and tendencies can be discerned in this rapidly expanding field of ethnoarchaeology. First, there was the work on traditional rural settlements in the Near East which continued Patty Jo Watson’s seminal study in Iran. Watson’s own *Archaeological Ethnography in Western Iran* (1979b) and Carol Kramer’s *Village Ethnoarchaeology: Rural Iran in Archaeological Perspective* (1982),

13 The wealth of book-length treatments of ethnoarchaeology in terms of monographs and edited volumes echoed the nineteenth-century importance of books as the prime format for scientific communication. Whereas journal articles played an equally crucial role, few subfields in processual archaeology produced as many volumes as ethnoarchaeology did.

as well as Frank Hole's work in Luristan belonged to that tradition. For obvious reasons, the dominant perspective in such studies was a direct-historical approach which relied on the assumed continuity between prehistoric past and Islamic present. Next, there was the ceramic ethnoarchaeology of scholars working in the American Midwest where individuals like Hill, Longacre, Stanislawski, and Deetz were involved. They walked in the footsteps of the 1960s research Hill and Longacre had been doing as graduate students under Lewis Binford. In a very general sense, the basic question was how traditional anthropological concepts like matrilocal residence, sexual division of labour, and particular kinship systems could be unravelled on the basis of ceramic associations with architectural units in the archaeological record. This sort of work was exported to the Philippines where Longacre inaugurated his Kalinga project in 1973.

Yet the domain where ethnoarchaeology was most active and innovative concerned the study of contemporary hunter-gatherers. This was largely the consequence of the Man the Hunter conference which had canonized the image of a universal hunting-gathering way of life and at same time indicated that the last representatives of it were rapidly disappearing. J. Desmond Clark, for example, emphasized at the Chicago meeting 'the urgent need for "ethnoarcheological" studies of such extant "Stone Age" groups while they still exist' (Clark 1968: 278) and he was the one who stimulated Lee to work on the !Kung. Since the available ethnographic work on hunter-gatherers was considered inappropriate for archaeological purposes (cf. Wobst 1978), archaeologists started to assemble their own data. There was not a moment to loose if one wanted to observe a nearly extinct lifestyle which had once been the quintessential human adaptation. Foraging societies being restricted to fairly marginal ecological zones, the emphasis was inevitably on fieldwork in the arctic and the tropics. In the first half of the 1970s, several scholars undertook ethnoarchaeological campaigns among contemporary foraging societies. Binford's work with the Nunamiut in central Alaska, Yellen's study of the !Kung San in the Kalahari desert of Botswana, and Gould's research with the Ngatatjara in the Western Desert in Australia all attempted to document what was seen as a quickly vanishing way of life. The first such substantial study to appear was John Yellen's ethnoarchaeology of !Kung in the Dobe region of Botswana, a study which was prompted by Richard Lee's cultural ecology of that region. Yellen focused on the patterns of !Kung settlements and sites, while simultaneously providing the reader with several detailed maps and raw data of this vanishing society of foragers. The book was hailed by Longacre as 'the best report of recent ethnoarchaeological research that I have seen' (1978: 359) and by Binford as 'the most detailed ethnoarchaeological study yet to appear' (1978c: 319). Meanwhile, Binford himself published several important articles on the Nunamiut organization of space, both at the local and regional level (1978c; 1980). These were flanked by two book-length treatments on 'faunal ethnoarchaeology': *Nunamiut Ethnoarchaeology* (1978b) which focused on the formation of bone assemblages in a hunting society and *Bones: Ancient Men and Modern Myths* (1981) which emphasized the extent of bone modification by human and nonhuman agents. He thus brought into ethnoarchaeology the theme of faunal analysis which he had

already been working on during the Mousterian debate. Very close to the *Bones* book was a work by C.K. Brain on early hominid remains in South-African cave sites: *The Hunters or the Hunted* (1981) which had also made of use actualistic, present-day studies of animal bone modification. Both books argued in fact that much patterning in faunal assemblages had been too eagerly interpreted in terms of hominid activities, so that effects of nonhuman agents like leopards, hyenas and porcupines had been underestimated. Whereas Brain attacked the osteodontokeratic culture Dart had defended for very early hominid cave sites in South Africa, Binford focused mostly on the home-base hypothesis which Isaac had cast as interpretations for the East-African living-floors. The unusual absence of certain anatomical parts in faunal assemblages was not necessarily the result of cultural selection but could be explained by differential body part preservation and carnivorous dietary preference.¹⁴ Furthermore, so-called cut marks very often appeared to be simple gnawing marks induced by carnivorous animals rather than intrepid hunters. Ethnoarchaeology of hunter-gatherer thus evolved seamlessly into the study of archaeological formation processes (Brain used the older name 'taphonomy') supported by ethology.

The study of natural and cultural formation processes affecting the archaeological record also formed the core of the work undertaken by Michael Schiffer (1976; 1987). Although ethnoarchaeology only played a secondary role in it, it often focused on the observation of processes between dynamics and statics in the present. It was only in a contemporary setting that the transformation from a systemic context to an archaeological context could be adequately followed. This was also the underlying rationale for the growing field of experimental archaeology: controlled experiments in the present allowed to disentangle the multiple variables responsible for producing certain known archaeological results. Although the approach was not new (flintknapping experimentation had been undertaken earlier in the twentieth century by Semenov in Russia, Bordes in France, and Crabtree in the United States), it received a renewed boost of attention with a number of large-scale experimental projects and general books (Ingersoll, Yellen and Macdonald 1977; Coles 1973; 1979). A final realm closely associated with to ethnoarchaeology was the incipient domain of modern material culture studies. Originally started for educational purposes, the study of material culture in present-day North-America was soon thought to elucidate the more general issue of formation processes and the relation between behavioural dynamics and material statics. Ascher (1968) had already turned to automobile graveyards in the 1960s, but the most important proponents of this approach became Michael Schiffer and particularly William Rathje whose Garbage Project in Tucson, Arizona went on for many years (Gould and Schiffer 1981; Rathje 1981). The study of formation

14 The idea that the differential body part representation in South-African cave sites resulted from carnivorous activities was first hinted at by Sherwood Washburn in the 1950s. Having attended the 1955 Pan-African Congress in Livingstone where Raymond Dart expounded the arguments for his osteodontokeratic culture, Washburn decided to study the bone modification by carnivores in the savannah today. He came to the conclusion that Australopithecines rather than successful hunters must have regularly been the victim of feline carnivores (Washburn 1957).

processes, experimental archaeology and modern material culture studies were related processual subfields which had all sprung from ethnoarchaeology's wish to understand the link between living context and resulting remains.

Ethnoarchaeology then was one of the most ramified fields during the heyday of processual archaeology. In fact, it touched upon nearly all themes of archaeological inquiry: early human origins, later Palaeolithic adaptations, origins of food production, Neolithic villages, protohistorical and early-historical settlements, urbanization and modernity. It stretched from the Australian desert to the Arizonan refuse dumps, and from the Alaskan cold to the Kalahari heat. It involved the very basic methodological issue of formation processes and the quintessential theoretical problem of how one went about from remains to reconstruction. It would go too far to say processual archaeology *was* ethnoarchaeology; but it is certainly true that without ethnoarchaeology there would not have been processual archaeology as we know it. Nevertheless, the central question remained: What did it all lead up to? How could the present still be used to understand the past after testing had been discredited?

Beyond analogy?

The ethnoarchaeological boom of the 1970s did not just bring about an intensification of an earlier interest in ethnography, but a fundamental reformulation of its purposes and objectives. Its inspirational function lost credibility now that there was no longer an independent means of verifying the source of inspiration. Its critical function, too, was no longer believed to be of great value. Several archaeologists were overtly dismissive about the sceptical papers: Longacre said that 'the "cautionary tales" that often result from this type of impressionistic work are not what we need' (1978: 363); Schiffer was 'becoming rapidly weary of the "cautionary note" format of these presentations' (1978: 242); and Kramer wanted to move 'beyond the rather bleak level of the "cautionary tales"' (1979: 6). Yellen was more subtle: even if such 'spoiler approach' was essentially negative in outlook, he still praised it 'as a valuable check on archaeological speculation' (1977: 8). Nevertheless, he too urged to go beyond this critical role in order to address more substantial and constructive functions of ethnographic analogy (1977: 6-12).

Claiming that a warning finger was not enough was all well and good, it remained to be seen in practice how one moved beyond the level of mere nit-picking. In the absence of straightforward testing, how could one further improve the nature of ethnoarchaeological inferences? Throughout the 1970s several solutions were favoured which deserve further discussion. They can be enumerated as follows:

1. improvement of testing through experimental archaeology and falsification
2. establishment of laws of human behaviour
3. establishment of causal understanding within the source (middle-range theory)
4. establishment of unambiguous and uniformitarian parameters

Improving the tests

The most obvious place to start concerned the very nature of testing itself, that is, it consisted of checking whether the weaknesses of the hypothetico-deductive approach could be remedied as such. A number of emendations were suggested. Ruth Tringham, for instance, agreed that the archaeological record was a poor context for testing an ethnographic hypothesis and suggested that such testing take place in an experimental setting where variables could be controlled (1978). In order to see what smudge pits had been used for, it sufficed to smoke hides and pots over experimental replicas to see which functioned best. The problem with such experiments was that they indicated the plausibility of certain interpretations but not their probability. In fact, it was already known from ethnohistorical sources that smudge pits could have been used in two different ways so that experimental archaeology did not unequivocally contribute to a more grounded functional attribution.

A second way of improving test procedures consisted of moving beyond the simple confirmation of hypothesized options. It was thought that if a test was to be truly accurate, it should not just verify the hypothesis but try to refute it. Stanislawski (1974: 20) admitted that '*scientific proof* is often impossible, and that *disproving* inadequate hypotheses is more reliable.' Murray and Walker asked for 'refutability' as 'an additional condition for the acceptance of analogical inferences' (1988: 261). In philosophical terms, the Hempelian hypothesis-confirmation had to make room for Popperian falsificationism: tests did not consist of enumerating as many confirming instances as possible, but of severely attempting to find refuting cases of the favoured hypothesis. Only if a hypothesis stood up against such attacks, could it be said to possess a certain degree of verisimilitude. The most explicit application of such falsificationist programme in archaeology occurred in the context of early hominid research in East-Africa, particularly in the work of Glynn Isaac (Isaac 1984; Blumenshine 1991). Although Isaac cannot be said to have been an ethnoarchaeologist in the strict sense (he did not undertake ethnoarchaeological fieldwork himself), his late-1960s interpretation of Plio-Pleistocene living floors in East-Africa as home bases for early hominid activities was considerably influenced by Richard Lee's study of the !Kung Bushmen in the Kalahari. Half a decade later, forced by Binford, serious doubts started to emerge about the value of this ethnographic input: 'Starting in the mid-1970s there has been a growing recognition that if we were not simply going to do prehistory by projecting the present into the past, then these interpretations needed rigorous investigation and testing' (Isaac 1984: 3). Such tests did not consist of seeking confirmation for the home-base model but occurred in 'a quasi-Popperian approach that actively attempts to overturn the initially favored hypothesis' (23). Together with his students Henry Bunn, Richard Potts and Pat Shipman, Isaac questioned whether the accumulations of bone and stone débris at sites in Olduvai Gorge and at Koobi Fora were really the remains of base camps where hunting and gathering hominids assembled and returned with the catch of the day. The simple co-occurrence of stone and bone at a site was no longer sufficient proof of the model if there were no clear signs of interaction between both. Clear evidence of cut

marks (the effects of stone tools on bones) and use wear (the effects of bones on stone tools) was therefore required to uphold the home-base hypothesis. Although eventually Isaac believed that his model withstood the tests, falsifying helped to liberate the model from its ethnographic roots so that it showed how the early hominid way of life ‘may have been very different from recent prehistory and modern times’ (66). Indeed, falsification allowed to move beyond ‘the tyranny of the ethnographic present’ (Wobst 1978) in order to find prehistoric uniquenesses.

This was also the core idea underlying all of Richard Gould’s publications at the time (Gould 1978b; 1980: 29-47, 138-141; Gould and Watson 1982). Despite his pioneering role in ethnoarchaeology, he vehemently opposed a reliance on ethnographic analogy: ‘The use of ethnoarchaeology to discover analogies to the prehistoric past is downright misleading’ (1980: 29). Instead, he sought to replace the argument by analogy with what he called ‘the argument by anomaly’. This consisted of two argumentative phases: first, one had to formulate a prediction about past behaviour on the basis of ethnographic analogy and uniformitarian assumptions; secondly, one had to check the archaeological record for instances which deviated from this predicted observation. Only such ‘contrastive approach’ (36), Gould argued, enabled the archaeologist to ‘know more about the past than one already knows about the present’ (32). The argument by anomaly tried to safeguard the uniqueness of the past from those archaeologists who tried to subsume all cultural behaviour under general laws and projective analogies. Gould accepted that uniformitarian explanations of the eco-utilitarian sort should be formulated first, but urged that cultural idiosyncrasies should be invoked to explain what was not covered by such laws. Though Gould made use of laws and analogies, the contrastive approach was meant to liberate archaeology from them. Gould’s eagerness to move ‘beyond analogy’ (the phrase returns in nearly all his writings) prevented him from realizing that his approach was still analogical and simply signified ‘a commitment to a “falsificationist” testing program’ of analogical arguments (Wylie 1982: 383). Indeed, in reviewing the famous Watson-Gould debate in ethnoarchaeology, Wylie has rightly argued against Gould that ‘an analogy by any other name is just as analogical’ (1982).¹⁵ Salmon agreed that replacing the argument by analogy with one by anomaly was ‘a mere terminological debate’ (1982: 75). What Gould did, in fact, was using analogies to generate hypotheses and seeking to falsify them by what he called ‘the contrastive approach’, ‘indirect reasoning’ or ‘the argument by anomaly’. Since he had narrowed the meaning of analogy down to projective inferences solely based on similarity (an equation, in fact, which held true for much of the historical usage of ethnographic analogy in archaeology), his approach which tried to study similarities *and* differences had to be given another name—although in the end it came closer to the proper logical definition of analogy.

15 The reason why I do not deal with this debate as a whole is that it was not really a debate at all. Watson stated her previous views, Gould reiterated his points, and in the end they agreed to disagree. Along with Wylie, the reader has to admit it was rather ‘disappointing’ (1982: 385). The ‘debate’ added no surplus value, so I have considered it more productive to discuss both positions separately.

Establishing laws

If some scholars wanted to improve the testing procedures by invoking experimental archaeology and falsification, others believed that a more fundamental rethinking of analogy was at stake. According to them, it could no longer suffice to study the context of verification if there was no attention for the generation of analogical arguments. Modified forms of testing sought to improve the transfer from source to target, but it was believed that the target itself, i.e. the relation between statics and dynamics should first be strengthened. This was where the attraction of law-building lured: if it could be shown that there were regular, law-like patterns between behaviour and material residue in the present, then one possessed a strong warrant for analogical inference. The staunchest defence of such approach was voiced by Michael Schiffer (1978) who blamed ‘the particularist orientation of ethnoarchaeology’: ‘The majority of studies, perhaps 90 percent, make no pretensions to general significance and predictably, achieve none’ (239). This particularism resulted from ‘the Boasian legacy [...] of glorifying the unique and eschewing the search for regularities’ (239). In contrast, he believed that the ethnographic present had to be used as ‘a fertile and appropriate laboratory for acquiring the laws needed to reconstruct the past from archaeological evidence’ (239). Finding general laws in the present was an efficient means to avoid analogy: it showed that the pattern was so universal that it could do without the specific instance of the source analogue. The pattern observed in the source was then only an exemplification of a much more general regularity. As a consequence, the amount of direct similarity between source and target did no longer matter; no special proximity between the analogue sides was required. Schiffer said:

Thus it should even be possible to derive laws applicable to the Paleolithic from study of Nacirema behavior [‘Nacirema’ being the well-worn exotic anagram for American]. For example, some of the principles that apply to nonsedentary social units, such as hunter-gatherer bands, may be acquired from study of backpackers, campers, and migrant farm workers. (Schiffer 1978: 240)

The study of modern material culture resulted directly from the confidence that cross-cultural laws of human behaviour existed and could be found. Schiffer believed that ‘the day cannot be far off when ethnoarchaeology will begin to supply the steady flow of laws needed’ (247). He, therefore, sent his students ‘among the Nacirema’ in order to find ‘laws governing manufacture, use, and even discard of particular artifact classes [and] basic principles relating to the general functioning of material culture’ (242). For instance, one of them observed how people dealt with broken clocks and came up with the ‘law’ that ‘as objects increase in size, the repairman tends to come to the object rather than vice versa’, being an instantiation of the general law that ‘as the mass of an element increases, there will be a decrease in the functional distance between the use and repair locations’ (245). No wonder that such ‘laws’ were ridiculed by Flannery as ‘Mickey Mouse laws’—they were hardly more than empirical generalizations on fairly trivial aspects, in fact so trivial that no ethnoarchaeological fieldwork was needed to come up with them.

Next to their banality, a more serious shortcoming of laws in archaeology was that they were rarely unambiguous. This was pointed out by Wylie (1982: 390): ‘It is unusual to find, or to be able to establish, a correlation between variables like human behavior and its material remains that is both absolutely necessary and exclusive such that it is possible to reason securely from consequent to antecedent.’ She further argued that the laws archaeologists claimed to have discovered were in the first place ‘laws of correlation’ rather than ‘laws of connection’, i.e. they indicated that certain attributes commonly occurred together (correlation) but provided no causal explanation of this regularity (causality).¹⁶ Such correlations might hold well across a number of contexts, but did not guarantee universal applicability. ‘Without any basis in an understanding of *how* these regularities are generated and *why*, or under what conditions, they may be expected to hold in other contexts,’ their predictive power remained weak (Wylie 1982: 386, original emphases). As a result, the uniformitarian criterion required for extrapolating those laws towards the past was not nearly met. It was not at all clear how contemporary inhabitants of Tucson who had a broken watch or a malfunctioning Westminster could tell us anything relevant about Palaeolithic behaviour in Europe. Most archaeologists maintained that such uniformitarian assumptions could only hold for the more mechanistic sort of historical processes (cf. Watson and Gould 1982: 362; 368).

Understanding the source

Law-building was an attempt to establish general correlations between statics and dynamics which were believed to hold true across the most variegated sources; it did not succeed in adequately understanding those links as it remained silent on the underlying causal processes. The latter was precisely what Lewis Binford envisaged with his ethnoarchaeological fieldwork among the Nunamiut: before extrapolating present patterns to the past, before establishing cross-cultural law-like generalizations, and in the absence of reliable independent testing, one first had to understand the causal, relevant patterning between dynamic behaviour and material results, that is one had to develop middle-range theory. He was very clear about this in the introduction of *Nunamiut Ethnoarchaeology* (1978b: 5-6):

The search for certain, relevant experience is the concern of this book. I am not directly involved in hypothesis testing. I am not involved in a direct way with the problem of explanations. I am concerned with sharing a series of concrete experiences sought in the hope of uncovering some of the links between an ongoing living system and the static archaeological products resulting from the dynamics of the situation.

In Binford’s view, ethnoarchaeology did not proceed by testing or law-building but required a thorough understanding of the vertical relations of causality. Though Binford kept historical particularism of the ‘muddle-headed’ Boas at

¹⁶ Interestingly, this goes straight to the heart of an old debate in philosophy where the empiricist Hume claimed that causality as such could never be seen but was a category read into the empirical phenomena by mental habit.

arm's length, his causality programme was as a matter of fact very close to Boas' demand for causal understanding in the comparative method. The neo-evolutionist sympathies notwithstanding, Binford's logic of analogy echoes Boas far more than Tylor. Like Boas, Binford was also wary about the possibilities of extrapolation; a causal understanding of the present entity was already difficult enough, let alone an inference from it about the past. But whereas Boas went further in his understanding of individual cultures (and gave up the ambition to extrapolate), Binford started to work on the least cultural aspects where extrapolation was still feasible.

Binford chose to focus on faunal assemblages because of the 'completely culture-free taxonomy of bones' (11). Human hunters were not actually making the bones but only 'partitioning, segmenting, and differentially distributing' segments of the animal carcass (11). Because the global anatomy of ungulates was the same in the present as in the Palaeolithic (in terms of sort and number of bones), one disposed of a uniformitarian anchor. Binford first attributed a 'general utility index' to the different anatomical parts of the caribou on the basis of their nutritive value of meat and marrow.¹⁷ He then investigated how this differential utility could explain the differential representation of bones at functionally distinct sites. He found that abandoned butchery sites were rich in skulls, antlers, maxillae and other poorly nutritive parts of the carcass that were left behind after partitioning the animal; base camps, on the other hand, had disproportionately large numbers of femurs and humeri, i.e. the bones associated with most meat and marrow that were introduced to the camp; while hunting camps where the least nutritious parts of fresh kills were eaten before transport to the base camp contained mandibles, upper forelimbs and limb extremities. This pattern provided 'the crucial linkage of behavioral dynamics and statics' (12). Binford believed it could be extrapolated towards the past:

My conclusion was that the formation processes of archaeological remains were indeed common to both contemporary and past eras. Many of the animal species present in assemblages are still extant, and the processes of exploitation and use operative in the past are still operative today. (Binford 1978b: 12)

On the basis of this research, Legge and Rowley-Conwy started to re-analyse the faunal remains from the site of Star Carr. The comparison of the differential presence of bones with the Nunamiat evidence suggested that the site must have been used as 'a hunting camp from where meat was removed to a base camp elsewhere' (Legge and Rowley-Conwy 1988: 94). This interpretation was diametrically opposed to that of Clark, the site's original excavator, who believed that it represented the remains of a community rather than 'the activities of a specialized group' (1954: 10). Interestingly, Clark too had come to his conclusion through an analogy with modern Eskimos: since skin-working was traditionally a feminine activity

17 Binford's assessment was not just based on the utility of every individual bone. He too incorporated the fact that a bone of lesser utility might 'ride along' one with higher utility if it happened to be anatomically associated to it. This was for example the case of the poorly nutritive phalanges which were attached to more richer parts of the hind legs.

in Inuit society, the remains of such activity at Star Carr suggested to him that women must have been present, next to men whose presence was attested by the remains of hunting equipment and tool manufacture. The occurrence of two sexes at the site led him to conclude he had to do with a community. The weakness of this interpretation resided in the fact that there was no uniformitarian ground to assume that the sexual division of labour among contemporary Inuit was the same for the Mesolithic inhabitants of Britain. On top of that, the weight of the conclusion ('the site was inhabited by a community') rested on limited similarity ('flint scrapers and ox-bone scrapers which suggested skin-working').¹⁸ And finally, there was no demonstration of an unambiguous relationship between the activity of skin-working or hunting and the sex performing that activity, let alone between the presence of both sexes on a site and the fact of having a community. The interpretation uttered by Legge and Rowley-Conwy, on the other hand, benefited from the fact that species like roe deer, red deer, elk and aurochs recovered at Star Carr had the same number of bones as the caribou and roughly the same distribution of nutritive parts across the carcass. The grounds for making a uniformitarian assumption were therefore much firmer. Of course, there was no reason to assume that the hunters at Star Carr exploited animal carcasses as sensibly and economically as the Nunamiut did but similar patterns of exploitation *were* found so that 'it would surely be perverse to argue that the similar patterns at Star Carr and Kongumuvuk [one of Binford's sites] result from different, culturally determined behaviours' (1988: 93).

Unambiguous and uniformitarian

The efficiency of analogical arguments, Binford reasoned, depended on the unambiguous links between statics and dynamics and the uniformitarian assumption between past and present. Not surprisingly, in his later work, he would turn to those aspects of the archaeological record where cultural impact was minimal and mechanical causation maximal: the physical modification of bone assemblages by human and nonhuman agents. Only in such fields could unambiguous and uniformitarian links be ascertained. His book *Bones: Ancient Men and Modern Myths* (1981) was an attempt at establishing criteria that distinguished human bone modifications like cut marks, breakage and chopping from animal modifications like gnawing, tooth marks and trampling—criteria that were later used in his analyses of the faunal remains of Klasies River Mouth in South Africa (Binford 1984a). As to unambiguity, he wrote: 'In a very essential way the

18 Clark's Eskimo analogy for Star Carr, despite Ascher's enthusiasm for it, was seriously deficient. At the base was the presence of flint scrapers which were interpreted as hide-working tools and the presence of certain enigmatic, heavily-polished ox-bones. Clark inferred that the latter too must have been used as hidescrapers because the Inuit used similar bones for working caribou skins (the illustrations which accompanied that analogy were rather unconvincing). Thus a first analogy was made for the functional interpretation of ox-bones as hidescrapers. A second analogy built on that previous one by saying that skinworking was a feminine activity in Greenland and thus at Star Carr. And finally, a third, if rather implicit, analogy was made when Clark reasoned that the presence of both men and women at a site implied by definition a community. As such, an entire community was projected at Star Carr largely on the basis of some polished ox-bones.

contents of the archaeological record must be viewed as products of a complex *mechanical* system of causation' (1981: 26, original italics). He underlined that he used the word causation in the strictest logical sense of a necessary, constant, and unique pattern of association, similar to the relation between an animal and its footprint. Such causation could only be observed in the present because the archaeological record contained only the caused patterns, not the causing processes: 'Only in the present can we observe the bear and the footprint together' (29). Ethnoarchaeologists had thus to find links between statics and dynamics that were both 'redundant and unambiguous' (26). Yet, Binford continued: 'An additional proposition must be met—that the same relationships obtained in the past as obtained in the present between bears and their footprints!' (27). This was the very essence of the uniformitarian assumption, which he called 'the interesting and important, perhaps crucial, problem archaeologists must solve' (27). As it provided 'intellectual anchors' between past and present, he contended that, like in geology where the term had originated, 'we must assume that knowledge gained from actualistic studies is relevant and applicable to the living systems of the past' (28, 27). Binford agreed that such was only feasible for certain classes of data: 'Ecological and anatomical characteristics of species still extant with which ancient man interacted were enduring objects for which uniformitarian assumptions might be securely warranted' (28). With this constriction of ethnoarchaeology to the most mechanical and immutable forms of patterning, we are far removed from the 1960s hypothetico-deductive optimism that welcomed any source of analogy as long as it was tested. As the strict separation of the context of discovery from the context of justification was no longer tenable, it did again matter where the analogy came from. The best proof of this change is that Binford opened the *Bones* book with a historiographic chapter. Once you admit that the context of discovery can never totally be superseded through independent verification, it becomes imperative that further research 'be carried out with an appreciation of the intellectual history of the field' (Binford 1981: 4). Whereas processual archaeology had always had a rather minimal interest in disciplinary history (like most fields dominated by a positivist epistemology), it was now admitted that the context of discovery was more than peripheral (cf. Pinsky 1989).

Place and population: a case study

The testing through experimental archaeology (Tringham 1978), the reliance on falsification (Gould 1980; Isaac 1984), the efforts at law-building (Schiffer 1978), the attempts at causal understanding of source analogues (Binford 1978b), and the search for processes with unambiguous and uniformitarian qualities (Binford 1981) were the main strategies summoned for improving the nature of ethnoarchaeological inferences. Because scholars worked in different fields of archaeological inquiry, it is perhaps useful to look at a small case-study in order to appreciate the differences between the approaches outlined above. In that respect, the estimation of population size from a given settlement of hunter-gatherers provides an illuminating example.

Naroll's constant and Bonnichsen's caution

Already in 1962, the demographer Raoul Naroll attempted to devise a formula for inferring the number of occupants of a given settlement from its covered floor space. On the basis of a cross-cultural sample of 18 modern cultures, he argued that 'the population of a prehistoric settlement can be very roughly estimated by archaeologists as of the order of one-tenth of the floor area in square meters occupied by its dwellings' (1962: 588). 'Naroll's constant', as the equation became popularly known, predicated the rise of ethnoarchaeological fieldwork and was strictly based on existing literature. As such, cross-cultural ethnography was invoked for the same inspirational purposes as in Ucko's study of mortuary behaviour, although it obviously went further in that it contained an early example of law-building. Inevitably, the logic of the New Archaeologists required that the predictions from such formula had to be independently tested against archaeological evidence. Bonnichsen's study of a recently abandoned Cree campsite in the Canadian Rocky Mountains provided a cautionary tale for the assessment of population figures on the basis of material remains (figure 9). Whereas he inferred on the basis of archaeological evidence that the site must have been occupied by 'two family groups using similar features' (1972: 283-4), one of its former inhabitants rectified that conclusion by informing that 'the permanent residents in the camp included Millie [the informant], her teenage daughter, and her eight-year-old son. On weekends Millie's husband and two adult sons came home from the Grande Cache coal mine where they were working' (285). The 'intuitive method of inferential reasoning' archaeologists relied upon had once more proven wrong (277).

Yellen's generalization

Ethnoarchaeologist John Yellen regarded such 'spoiler' approaches as insufficient but realized at the same time that the requirements of hypothetico-deductive testing were 'overly restrictive, given the incomplete nature of most archaeological data and the difficulty of "proof"' (1977: 6). On top of that, Yellen argued that Naroll's constant had only limited application for Palaeolithic contexts where the essential variable of 'covered floor space' could only be guessed at. His ethnoarchaeology of the !Kung San, therefore, attempted to devise a criterion that was more relevant for foraging societies with simple and perishable living structures. From the 15 !Kung camps where both social and spatial variables were known, he calculated that the size of the space between the huts (the 'inner ring' or hut circle) co-varied with the number of occupants, and that the size of the surrounding space (the 'outer ring' where activities areas were found) correlated with the length of occupation (figure 10). Using these correlations, he devised a number of equations which had to enable the Palaeolithic archaeologist to predict the population size and duration of occupation of a given site. Yellen named this model 'the single most important contribution' he had made but admitted at the same time that 'in the event that [archaeological] data do not fit into this mold, I am uncertain just what the next step should be' (131). The reason for this was, that despite his interest in the 'underlying mechanisms' of the given correlations (101), he had re-

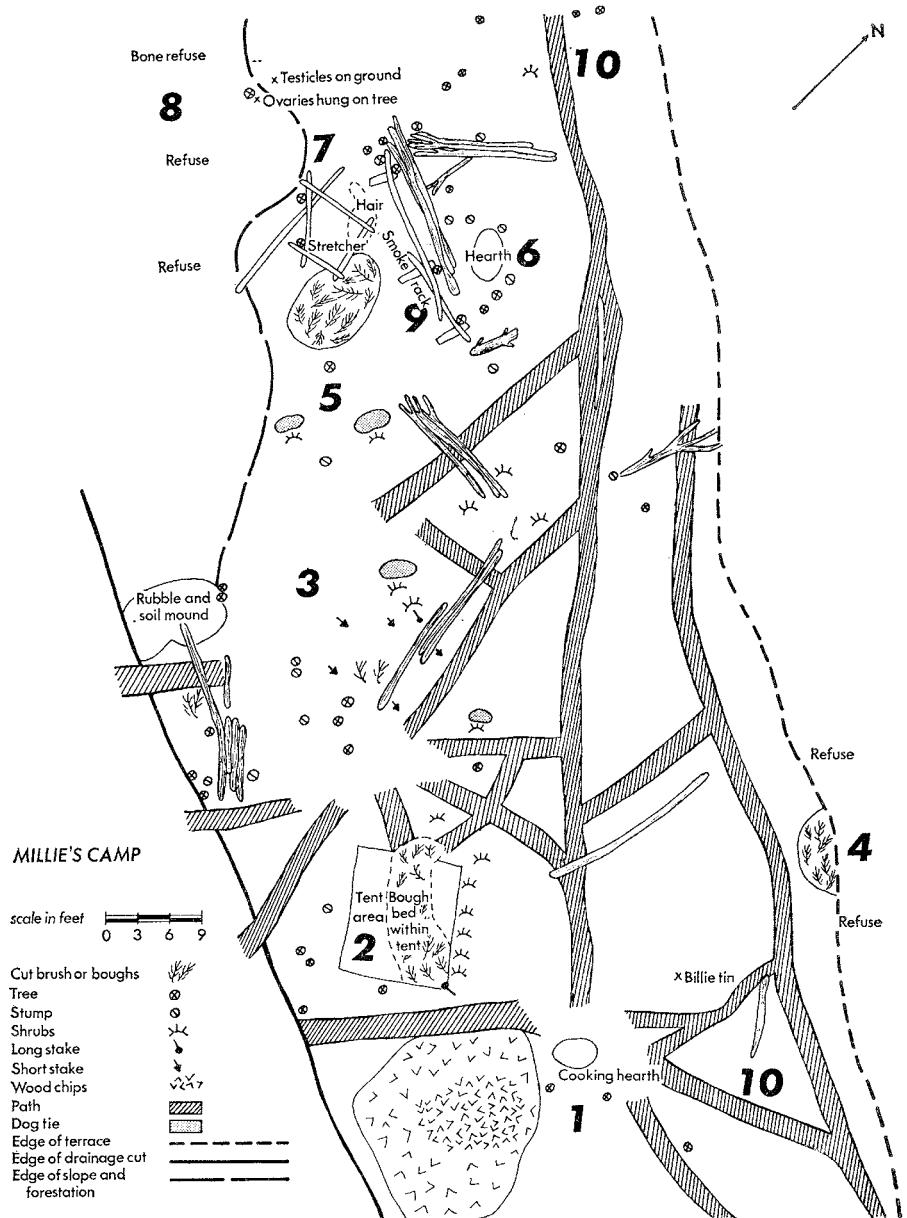


Figure 9. Bonnichsen agreed that the archaeological record reflected social behaviour but in his interpretation of Millie's camp, a recently-abandoned Indian site, he came to the conclusion that the archaeologist's conclusions were often incorrect. Early ethnoarchaeology served a cautionary function (Bonnichsen 1972: figure 2)

frained from unravelling the relevant causal processes. Yellen's work was therefore an attempt at law-building in the sense of using empirical generalizations from ethnography to make predictive equations for archaeology on the basis of behaviour-material correlations whose causality was not further specified.

Binford's causes

According to Binford, Yellen 'never isolated a cause' (1978c: 322). Binford vehemently criticized the !Kung ethnoarchaeology: 'This is a description of the way the world is or appears. It is not an explanation' (321). In his paper 'Dimensional analysis of behavior and site structure' (1978c), Binford applied Yellen's equations to an Eskimo hunting stand in Alaska and found that they gave very wrong results compared to what he had observed ethnoarchaeologically. Binford did not see the Nunamiut as deviations of the general rule; instead he contended that 'some "other things" are causing the Nunamiut case'. The understanding of these causes was the gist of Binford's work in the late 1970s:

I have argued that empirical generalizations, no matter how complicated (as, for example, Yellen's observations on site size and group size and occupational duration), are what we seek to understand, and only with understanding can we anticipate how observations will vary under changed conditions. The latter is, of course, what we mean by predictions. Our ability to anticipate variability in the world is in turn a measure of our understanding. (1978c: 323)

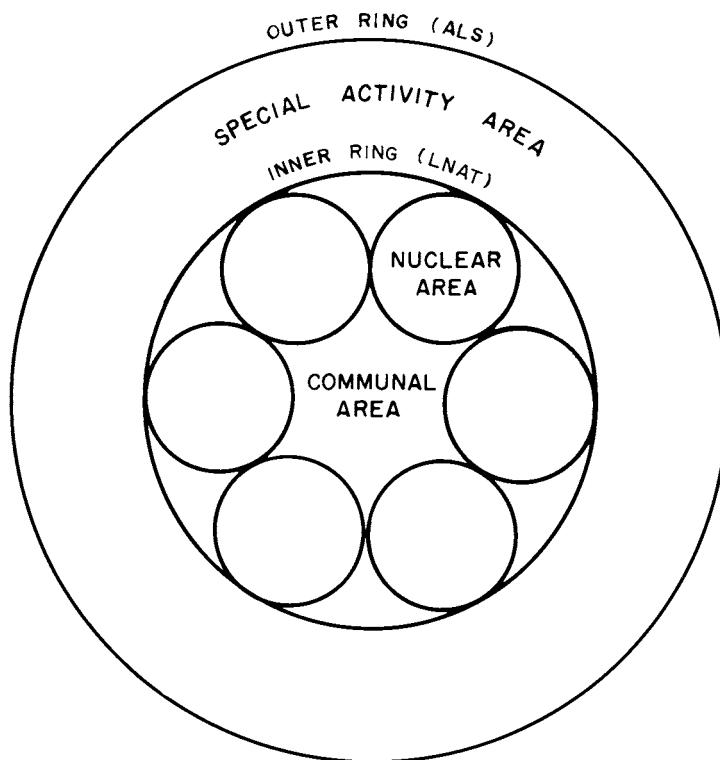


Figure 10. Yellen studied !Kung settlements to extract generalizations about hunter-gatherer spatial behaviour. He reasoned that the communal area would increase with the number of huts (nuclear areas). The inner ring was thus believed to be an expression of group size. The outer ring was seen as a function of the time during which the settlement was used, since this is where special activities took place (Yellen 1977: figure 12)

Binford went into great efforts to disentangle the relevant variables which had been responsible for the site formation at an Eskimo hunting stand (figure 11). He had spent several days in the early 1970s describing Nunamiut behaviour at such game observation point and inventorying the material remains left behind between the glacial boulders that surrounded the site. All sorts of measurements like the distance of each hunter to the fire and the tossing distance for sardine cans were taken to find out the relevant behavioural causes for archaeological site formation. He argued that the sort of activities carried out at the site, the technological organization of those activities, their spatial organization, and the form of disposal of material items (like tossing and dropping), formed an intricate and complex web of explanatory variables. At several places in the paper, Binford started to play with those variables in order to see the effects on the archaeological record. ‘Let us imagine,’ he said for example, ‘no marrow bones are being eaten, only dried meat’ (318). Bone chips and splinters would thus be absent, so that the drop zones in front of the individuals would be fairly empty, the highest density of materials being found in the toss zones behind the occupants, so that disposal areas would be better represented than activity areas. Like a puppeteer, Binford pulled the relevant strings in order to see how the archaeological record reacted. The paper was not a methodological statement (although the terms ‘drop zone’ and ‘toss zone’ gained widespread attention) but ‘an exercise in theory building’ which presented some personal ideas:

They are not empirical generalizations. I am not offering inductive arguments or arguments from ethnographic analogy. *I am not saying that all men will conduct the same activities in hunting camps. I am not saying that all men will play cards in sites with glacial boulders in them. I am not saying that all target shooting was normally conducted away from the group because of the noise of rifle fire! I am saying that my study has prompted my imagination. I have been able to imagine patterns of interaction among variables which could result in different patterning in the archaeological record.* (1978c: 320, original emphases)

Binford had arrived at a causal understanding of site formation in one ethnographic source, but refused to project it towards an archaeological target because it lacked a uniformitarian ground. His original intention had first and foremost been on the level of middle-range theory, i.e. the study of statics and dynamics in an ongoing system; in the absence of uniformitarianism, extrapolation to the past was simply not an option. The challenge had been one of ‘reducing ambiguity and increasing the accuracy with which we may analytically identify past causes of variability in the archaeological record’ (Binford 1984b: 255). Whereas Binford increasingly turned to data sets where the criteria of unambiguous causation and uniformitarian assumption were more easily met, the result of his ethnoarchaeology showed that the estimation of population size on the basis of ethnographic data was far more complex than the application of a simple formula.

O'Connell's source variety and Gould and Yellen's predator theory

The debate on site structure continued unabated in the 1980s. Binford's student James O'Connell studied spatial patterning among the Alyawara, an Australian group of hunter-gatherers (1987). He tried to disentangle the relevant determinants of Alyawara site structure but went further than Binford's study of the Eskimo hunting stand by comparing his case with the site structure of two other ethnoarchaeologically known hunter-gatherer societies: the !Kung and the Nunamiut. He thus looked for relevant dimensions of variability that were applicable beyond a single ethnographic instance. !Kung site structure was found to be 'very similar' (99) to the Alyawara system in terms of an internal organization

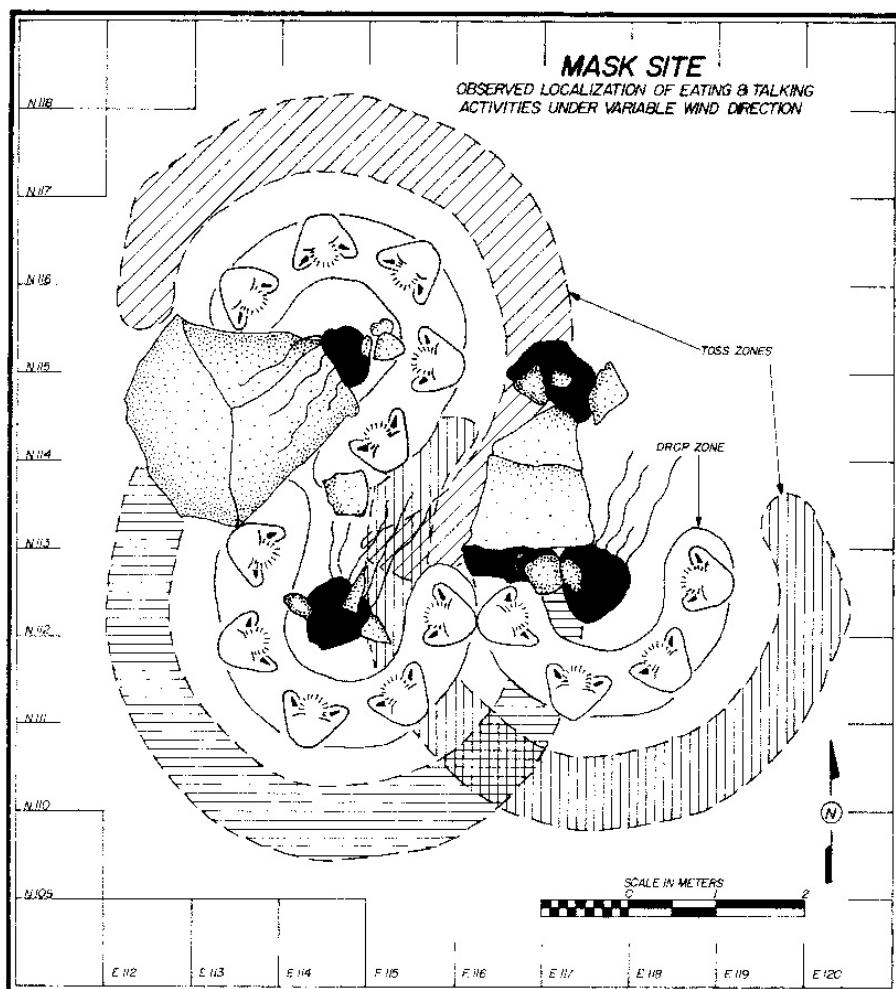


Figure 11. Binford argued that before the present source is extrapolated towards the past, it should be properly understood, even in the most minute details. His study at Mask site, a caribou hunting stand used by contemporary Nunamiut, disentangled the factors responsible for the production of the archaeological record, such as wind direction, type of crafts, activities and number of occupants (Binford 1978c: figure 21.5)

around nuclear household areas and a lack of spatial segregation of activities, the only difference being that the distance between households was considerably larger among the Australian aborigines. On the other hand, the Nunamiut represented a very different pattern: there was a distinction between winter and summer camps, activities were often spatially segregated, and internal differentiation was higher. O'Connell tentatively inferred that most of the variability in site structure between the foraging societies under investigation was a function of the degree of reliance on food storage, seasonal variation in weather, household population size, and the length of occupation. He coupled his conclusions to Binford's (1980) earlier distinction between the settlement systems of forager versus collector economies of hunter-gatherers. Whereas foragers like the !Kung relied on immediate consumption of food assembled during the radial foraging trips from a base-camp, collectors like the Nunamiut relied on storage of very large quantities of food that were seasonally obtained. The former gave rise to a fairly undifferentiated settlement system of central places with only minor differences between them; the latter resulted in a complex system of residential sites next to special activity sites (such as hunting stands, kill sites, butchering sites, and caches) with substantial differences in spatial layout and faunal representation. O'Connell thus attempted to delineate the causal processes responsible for the observed variability among hunter-gatherer use of space.

In a paper published the same year, Gould and Yellen (1987) focused on the differences in household spacing between the !Kung and Western Desert Australian aborigines. How come, they wondered, that with foragers living in similar arid conditions shelters and huts are placed on average only 7 m apart among the !Kung and more than 35 m apart among the Australian aborigines (a pattern that had also been noted by O'Connell)? They put forward six different causes of this variability in residential spacing (kinship, the degree of sharing, the length of occupation, the size of the household, the body size of prey, and predation), and selected the latter, fear of predation, as the key to understanding the observed patterns: in the Kalahari desert all large southern African predators like lions, leopards, hyenas, and cheetahs were present; in the Western Desert of Australia these were entirely absent. Not unlike O'Connell, Gould and Yellen tried to distil the relevant causality of settlement spacing that was applicable in several distinct hunter-gatherer contexts. Their hypothesis, however, was fiercely attacked by Binford who came up with numerous counterexamples of tight spacing in Australia and loose spacing in the Kalahari, of high residential density in predator-free contexts and low residential density in predator-rich environments, all of which were meant to demonstrate 'the inadequacy of the "predator fear" argument' (1991a: 268). Binford indicated that the relation between faunal environment and settlement behaviour was far more complex, a lesson which went hand in hand with his preference for the more uniformitarian matters of the archaeological record. Site structure could not be reduced to a single environmental variable but required an appreciation of the multiple dimensions underlying cross-cultural variability. However, what we see in the work of O'Connell, Gould and Yellen is the ambition to go beyond to study of a single ethnoarchaeological

case in order to increase the number and variety of source contexts from which to reason.

Whitelaw's world sample

This was the point of departure for Todd Whitelaw's work on community space among hunter-gatherers. Against Binford's obsession with mechanistic aspects of material causation, Whitelaw wrote:

While processual archaeology directed toward the development of middle range theory has stuck to a relatively safe path by addressing patterns in material behaviour for which a uniformitarian assumption can be made, the range of questions for which such an approach is fruitful appears to be fairly limited. (Whitelaw 1991: 183).

Whitelaw believed that a different path in actualistic studies could be less restrictive and even 'involve inferences about past individuals' perceptions and decisions where there may be more than one potential pattern of behaviour' (139). How could one alleviate the uniformitarian and unambiguous requirements and at the same time maintain a high degree of inferential confidence? Whitelaw's answer to that question was fairly ingenious and merits some closer attention. First of all, on the basis of ethnohistorical, ethnographic and ethnoarchaeological publications he assembled a world-wide sample of about 800 hunter-gatherer settlements from 112 known foraging societies for which a good plan and an estimate of population size could be obtained (or at least reconstructed).¹⁹ He then studied the patterns of settlement spacing and the demographic figures to infer occupation density (figure 12). Not denying the reality of differences between cultures, he demonstrated that these differences could be seen 'as systematic variations within different social and ecological contexts' (181). The determinant variables for settlement spacing he found were kinship (close kin tended to live close to each other), group size (larger groups showed lower occupation densities), ecological context (subarctic and arctic groups showed low occupation density, groups living in savannah or tropical contexts had higher densities), and the degree of sedentism (residential spacing increases with the length of occupation). Just like Binford, O'Connell, and Gould and Yellen had done before him, Whitelaw tried to disentangle the relevant variables of the complex phenomenon of settlement spacing. Just like them, too, he did not rest with just indicating correlations between certain variables but sought to define the underlying causes. For instance, he noted that the ecological variable in residential spacing had to be understood in terms of whether or not a certain biome encouraged cooperation in subsistence-related activities. In desert contexts, foragers mostly relied upon plant foods which are gathered and consumed individually so that settlement spacing tends to be rather large. On the other hand, in tropical biomes, meat becomes a major dietary component

19 Particularly well documented communities even provided further information on subgroups and discrete spatial clusters, amounting the sample to a total of 1762 different social situations (Whitelaw 1991: 141).

which requires maximal cooperation both in acquisition (hunting) and in consumption (collective and immediate consumption is imperative as preservation conditions are poor); occupation density is thus very high. Although arctic foragers equally subsist upon animal resources acquired through collective hunting, the possibility of storage by drying and freezing meat minimizes the need for shared consumption, thus encouraging individual consumption and residential spacing. Whereas Binford had only outlined the causal patterns for one particular site and O'Connell, Gould and Yellen for only a handful of hunter-gatherer societies, Whitelaw went further and devised a set of globally applicable processes. The differences between the !Kung and the Nunamiut social use of space which made Yellen's formula inappropriate for Binford's analysis were now accounted for by the same set of behavioural rules. Whitelaw had expanded Binford's quest for a causal understanding into a global, cross-cultural system of foraging adaptation. Generally parsimonious with compliments, Binford applauded Whitelaw for having 'most elegantly laid out' the problem of site structure (1991b: 25); 'Whitelaw's work is important' because he 'has isolated some truly provocative patterning' (1991a: 274, 269). Unlike Schiffer, Whitelaw did not seek homogenizing cross-cultural laws of human behaviour which were universally valid and uniform but tried to account for cross-cultural *variability* in human behaviour.

What was the archaeological value of this work? How could the patterns of settlement spacing of the few remaining hunter-gatherers shed any light on the range of prehistoric behaviour which was presumably much more diverse? Although Whitelaw acknowledged the role of social factors like kinship and group size, he opted for 'an explicitly ecological perspective' because this allowed 'the development of expectations of variation in behaviour, based on different patterns of

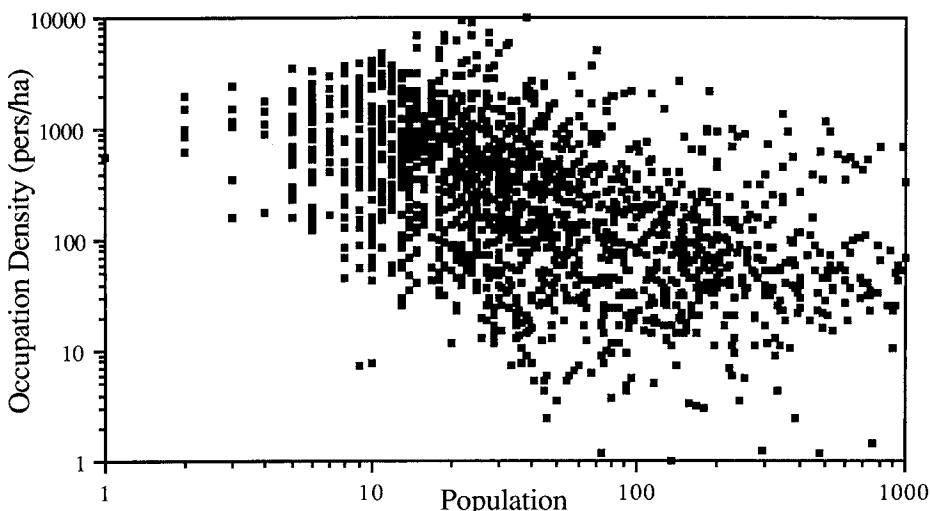


Figure 12. A worldwide ethnographic sample assembled by Whitelaw shows a general tendency between community population and occupation density. Whitelaw dissected this 'cloud' into various ecological zones and sought to account for the resultant patterns (Whitelaw 1991: figure 4)

subsistence exploitation' (183). Given the basic ecology of a region, one could make a prediction on settlement spacing from Whitelaw's model. This did not lead to a simple projection because the other side of the equation, i.e. settlement spacing, could be independently assessed in the archaeological record. Both ecology and residential spacing were archaeologically observable variables. Therefore, Whitelaw's model allowed 'to cope with differences in the context between the source and subject of analogies, such that our reconstructions will be context-specific, and not simply a projection of the same simple model onto the past' (183). In fact, the model 'could be falsified through comparison with the excavated evidence'²⁰ so that uniqueness of the past was recoverable 'beyond the limited sample of human behaviour actually documented historically and ethnographically' (183). As such, Whitelaw's *tour de force* integrated Gould's claim for recognizing archaeological 'anomalies' through falsification, Schiffer's search for cross-cultural principles, and Binford's insistence on causal understanding, while at the same time making use of settlement data provided by the ethnoarchaeology of Yellen, Gould and others. Although the precise estimation of population figures was not directly what Whitelaw's model envisaged, the relation between social and spatial behaviour in foraging societies had been greatly elucidated by benefiting from two decades of intense debate on the nature of ethnographic analogy.

Source and subject-side strategies

From Bonnichsen's cautionary tale to Whitelaw's model, the debate on site structure reveals how ethnoarchaeology has gone a long way in discussing logical difficulties and enhancing interpretative possibilities from the argument by analogy. The enormous growth of ethnoarchaeological studies in the second half of the 1970s, therefore, was more than a scaling-up of the New Archaeology's interest in anthropology but entailed a profound rethinking of the concept of ethnographic analogy. The two main functions of analogy in the late 1960s—inspirational and critical—paled into insignificance at the moment ethnoarchaeological fieldwork was undertaken at a much larger scale than ever before. Once the deficiency of independent testing as a means of hypothesis-evaluation had been demonstrated, the use of ethnography could no longer be said to be simply heuristic; what was the point of having a profusion of ideas if there was no means of evaluating them? Likewise, the genre of the cautionary tale lost attraction; again, what was the point of repeatedly stressing the difficulty of interpretation without trying to remedy it?

At the cutting edge of modern ethnoarchaeology scholars strove to go beyond such minimal functions and were forced to address the issue of analogy. Whereas archaeologists in the previous decade had been at pains to circumvent analogy, now they approached it frontally. Following Wylie, their approaches can be classified into 'source and subject-side strategies for establishing relevance' (1985: 100).

20 Such falsification was only possible for well-preserved 'snapshot sites' with assumed residential function (such as certain sections of Pincevent). The more complex (and much more frequent) palimpsests are of little help here.

Processualists realized in practice her device that ‘archaeologists must work aggressively at both sides of the analogical “equation”’ (101). At the subject side (or target side as we would call it), analogical inferences were strengthened by improving the procedures of archaeological testing. The most momentous of such strategies consisted of monitoring analogical predictions into falsificationist channels as this allowed to detect unique features of the past that were ethnographically unknown. Falsificationism in archaeology originally started as a means to enhance testing but soon developed, particularly in the archaeology of African hominids, into entirely new fields of research like taphonomy and site formation processes. The principal ambition was the elucidation through actualistic research of the relevant processes responsible for the formation of the archaeological record. At the source side, we find strategies like law-building, causal understanding and the uniformitarianism-unambiguity programme of research. Despite their typical differences, all these approaches shared a focus on the relevant processes between statics and dynamics in the source. Schiffer sought universal correlations in the present world between the systemic context of behaviour and the archaeological context of material remains. Binford tried to understand such correlations in a causal way, first in an explanatory fashion which was strictly confined to a particular situation in the present, but eventually in an extrapolating fashion that was based on the principle of uniformitarianism. Whitelaw attempted to devise a cross-cultural set of variables responsible for a particular form of material behaviour that could in principle be falsified archaeologically. In sum, then, the most striking feature of such source-side strategies was their universal attention to the relevant relations of causation. An ethnographic analogy could only be successful if due consideration was first given to the dynamic processes in the source from which the analogy was to be drawn. Actualistic research in taphonomy had worked from the same principle: first understanding present processes, then turning to past patterns. While traditional ethnographic analogies had universally relied upon the horizontal relation of similarity, the great merit of ethnoarchaeology was its focus on the vertical relation of causality. The issue of relevance thus surpassed the amount of similarity as the prime criterion for analogical reasoning.

Whereas target-side strategies enhanced the empirical content of analogical arguments, source-side strategies were more directed at improving the logical structure of the analogy. Next to the consideration being given to relevance, a number of other strengthening criteria received further attention. Whitelaw’s sample of 800 settlements from 112 different foraging societies, brought the *number of source contexts* well beyond the ca. 50 known settlement plans from about fifteen hunter-gatherer cultures. Similarly, Schiffer’s attempt to draw laws from contexts as diverse as modern North-America and contemporary hunter-gatherers shows how the *variety of source contexts* was appreciated as an important criterion. Finally, Gould’s interest for anomaly and the falsification Isaac and Whitelaw called for are indicative of the place *dissimilarity* could take in their analogical arguments. Once that formal similarity was no longer the sole criterion, a certain discrepancy between source and target could be easily accommodated and even welcomed.

The ethnoarchaeology of the late 1970s, then, genuinely moved beyond the criterion of formal similarity. Indeed, it was generally acknowledged that 'the lifestyles of prehistoric hunting and gathering groups are not necessarily or even likely replicated by recent surviving counterparts' (Yellen 1977: 4). This awareness of difference did not lead to despondency. Quite on the contrary, it served as a challenge for inventive work on the structure of analogical reasoning. Had culture-historical archaeologists been at pains to find modern analogues that were as close as possible in time and space to the prehistoric culture under consideration, processual ethnoarchaeology blossomed, not despite the agreed upon distance between source and target, but thanks to it. Contemporary societies functioned therefore as imported analogues rather than as manifest analogues (Leatherdale 1974).

Concomitant to the substantial work in ethnoarchaeology was a greater positive appraisal of the place of analogy and the logic it involved. A couple of publications in the early 1980s explicitly discussed the logical structure of archaeological analogies from a philosophical point of view (Salmon 1982; Watson, LeBlanc and Redman 1984; Wylie 1982 and especially Wylie 1985). Salmon defended the logical point that 'analogical arguments are not intrinsically weaker than any other inductive arguments' (1982: 79). Watson, LeBlanc and Redman, once the staunchest advocates of deductive reasoning in archaeology, now accepted that 'the basic principle of all archaeological interpretation is analogical,' even if such reasoning required 'an inductive leap' (1984: 259, 260). And Wylie held that 'though the argument by analogy is inevitably liable to error, it can be closely controlled and highly discriminating with regard to dissimilarities between past and present' (1985: 107). All authors agreed that the amount of similarity was a relatively unimportant aspect of analogical reasoning. A good analogy did not require that source and subject be identical: 'archaeologists do not argue that past cultures are exactly similar to present ones' (Watson, LeBlanc and Redman 1984: 261). Both Wylie and Salmon described at length the logical criterions for assessing and strengthening analogical arguments and they both emphasized the all-important criterion of relevance: 'Relevance is the most important consideration in assessing the success of an analogical argument' (Salmon 1982: 82). Wylie drew the valuable distinction between formal and relational analogies whereby only the latter were explicitly based on considerations of relevance. Formal analogies were inevitably weaker since they strictly relied on formal similarities without an awareness of causality. Relational analogies, on the other hand, were more interesting since they involved 'a demonstration that there are similarities between source and subject with respect to the causal mechanisms, processes, or factors that determine the presence and relationships of (at least some of) their manifest properties' (1985: 95). These logical reflections, in fact, reformulated in a more abstract language one of the most fundamental changes that had occurred in ethnoarchaeology, i.e. the idea that proximity between source and target was not an absolute, nor a sufficient criterion.

Decline and fall of ethnoarchaeology

From the discussion on hunter-gatherer site structure, it becomes clear that ethnoarchaeology did not die out after the triumphant semidecade from 1977 to 1982. An increasing number of archaeologists spent some time in the ethnographic field, joint the discussion, and contributed to the amassing wealth of ethnoarchaeological literature. Had the studies on contemporary foragers originally been limited to the !Kung and the Nunamiut, now new fieldwork by Binford and O'Connell brought the Alyawara (Australia) and the Hadza (East-Africa) into the picture during the 1980s (Binford 1984c; 1986; 1987; O'Connell 1987; O'Connell, Hawkes and Blurton Jones 1988a; 1988b; O'Connell and Marshall 1989). Likewise, Yellen's work on the !Kung was continued, expanded and followed up by scholars like Polly Wiessner (1983) and Susan Kent (1993). The same holds true for some other projects outside the hunter-gatherer realm: Longacre's ceramic study in the Kalinga area (Philippines) became the longest continuous investigation in ethnoarchaeology where several generations of American graduate students from the University of Arizona did research (Longacre 1991; Longacre and Skibo 1994). On top of that, ethnoarchaeology remained no longer something of a North-American monopoly now that British scholars (like Ian Hodder and Henrietta Moore) and French scholars (like the Petrequins and Alain Gallay) joined the ranks.²¹

The field benefited furthermore from the logical and philosophical expositions on the proper use of analogy further (Salmon 1982; Wylie 1982; 1985). Inductive logic was consulted to understand the internal mechanism of the argument by analogy, strength criteria were enumerated and checked, and the importance of formal similarity was shown to be secondary to relevant causality. Though analogies were admitted to be inductive arguments which are inherently weaker than deductive reasonings, the general opinion was that they are not all equally valid but can be assessed in terms of relative strength. One of the principal consequences of this logical clarification was that analogy was no longer considered the Achilles heel of ethnoarchaeology but became the accepted basis for all ethnoarchaeological inference. Authors with the most divergent theoretical agendas agreed on this point when they exclaimed that 'analogy is a basic and fundamental

21 Alain Gallay, a Swiss by birth, has made a large impact in French academe (cf. *Ethnoarchéologie: Justification, Problèmes, Limites: XIIe Rencontres Internationales d'Archéologie et d'Histoire d'Antibes*. Editions APDCA, Juan-les-Pins, 1992). It deserves to be noted that in general the French archaeological tradition has been rather reluctant towards the use of ethnography (Sackett 1981). Whereas Parisian *académiciens* in the early eighteenth century had been instrumental in drawing upon ethnography to understand flint tools, this interest was bracketed during the following centuries. Remember how De Mortillet's *paléoethnologie* was an attempt to write an ethnology of the Palaeolithic precisely *without* recourse to actual ethnology. Similarly, Leroi-Gourhan has systematically called for *une ethnologie préhistorique* in his interpretation of the Magdalenian site of Pincevent, that is, an ethnological reconstruction of the prehistoric past solely on the basis of prehistoric evidence. François Bordes, the other leading French prehistorian of the mid-twentieth century whose geology-inspired research programme differed profoundly from Leroi-Gourhan's more historical approach, disavowed the use of ethnography with a similar vehemence.

tool of middle-range research' (Binford 1987: 505) or that 'all archaeology is based on analogy' (Hodder 1982a: 9).²²

Although initially continuing the work of the processual heyday, in the long run ethnoarchaeology also underwent major changes in terms of aims, methods and functions. To understand these changes, it is advisable to look first at what happened to the two classical themes of the Binfordian research programme in hunter-gatherer ethnoarchaeology: the study of site structure and faunal analysis.

The isolation of hunter-gatherer ethnoarchaeology

Binford's Nunamiut research remained the most influential ethnoarchaeological study on hunter-gatherers throughout the 1980s. In a recent review essay, James O'Connell (1995) identified the work on site structure and faunal remains as two of the most prominent subfields in modern ethnoarchaeology as they had been the focus of very intensive research and played a crucial role in Palaeolithic studies. Both themes went back to the Nunamiut publications. The discussion on site structure, as described in the previous section, built further upon Binford's articles 'Learning from an Eskimo hunting stand' (1978c) and 'Willow smoke and dogs' tails' (1980). The discussion on faunal remains was to a large extent provoked by Binford's monographs on differential body part representation (*Nunamiut Ethnoarchaeology*, 1978b) and bone modifications (*Bones*, 1981). O'Connell's article, which is in the first place a long bibliographic essay, gives an impression of the masses of work undertaken in these and closely related fields: the bibliography counts up to more than 400 references (404), 90 % of which were published after 1980. It includes papers with themes varying from a study of 'variability in long bone marrow yields of East African ungulates' to an 'ethnoarchaeological model for the identification of prehistoric teepee rings in the boreal forest'. Once restricted to a discussion between Binford, Yellen and to a lesser extent Gould, hunter-gatherer ethnoarchaeology had now become a wide-ranging, multifarious, and complex field of debate which witnessed an enormous burst of activity during the next fifteen years.

Inevitably, this boost of attention led to an increasingly specialized field. This can already be noted from the format of publishing research results: had Binford, Yellen, Brain and Gould been able to expound their views in book-length treatments which attracted a relatively wide readership, now most studies appeared as technical articles (often multi-authored ones) in specialist journals like *Journal of Archaeological Research*, *Journal of Anthropological Research*, *Journal of Anthropological Archaeology*, and *Journal of Archaeological Science*. What had once been a general debate on the nature of archaeological inference came to be a highly technical field of specialist expertise where publications were no longer the product of one individual's pen and creativity, but the result of collective teamwork efforts. This was less the case for the discussion on site structure which continued as a fairly homogeneous debate where only more dimensions and more ethnographic data were drawn in than it was for faunal analysis. The initial work

22 But see Kent (1987) for one of the rare, more sceptical voices.

by Binford and Brain was carried on by a host of scholars like Pat Shipman, Dian Gifford, Lee Lyman, Richard Potts, Henry Bunn, Anna Behrensmeyer, Robert Blumenschine, and Gary Haynes and developed into one of the most technical and diversified realms of archaeological research.

Firstly, the research on bone modification by human and nonhuman agents had originally kicked off as part of the broadly relevant question of early hominid diet and the role of hunting therein (Gifford 1981; Lyman 1987): Isaac affirmed that the accumulations of bone and stone in East-African sites were home bases of hunting hominids; Binford denied such claims (and Brain did the same for the South-African cave sites); Isaac's students started to falsify the propositions (by distinguishing stone tool cutmarks from carnivore tooth scratches and rodent gnawing marks; cf. Shipman and Rose 1983); originally they reaffirmed the original hypothesis for early hominid hunting, butchering and carcass processing at Olduvai Gorge and Koobi Fora (Bunn and Kroll 1986) but Binford systematically criticized such claims. Had the discussion originally been on the role of meat in early hominid subsistence, now authors vehemently disagreed on issues whether hominids had exploited carcasses before or after hyena scavenging (Binford, Mills and Stone 1987; cf. Binford and Stone 1986). Parallel to that, the requirement of technical skills on the part of the researchers became more stringent: macroscopic inspection was replaced by observations obtained through a scanning electron microscope; analytical categories for describing bone modification became increasingly minute; knowledge on carnivore and rodent ethology, chemical composition of bones, geomorphological abrasion and whole range of other fields became prerequisite. Interdisciplinary cooperation between multiple scholars with varying scientific backgrounds became the new role model. The study of bone modification thus developed into a highly sophisticated, natural science branch of Palaeolithic research.

Secondly, the study of differential body part representations started from a similar straightforward question and devolved also into a technical conundrum where causes and consequences became increasingly difficult to disentangle. In Binford's original work, the nutritional value of a skeletal part (expressed in terms of meat and marrow utility) was the main determinant of the presence or absence of it on certain types of sites. Later research showed that much more factors were in play such as prey size, transport facilities, storage possibility, mode of cooking, form of sharing, predator action, etcetera (Yellen 1991a, b; Kent 1993; Lupo 1995). Scholars were at pains to distil further dimensions that bore on the production of faunal assemblages to the point that an explanatory model became harder and harder to reach. The puppet of the archaeological record seemed to be steered by much more strings than previously expected; the relevant mechanism became increasingly difficult to disentangle: 'many of the principles are not clear-cut and do not provide simple links to reliable reconstructions of the past,' some authors had to grant (Bunn, Bartram and Kroll 1988: 453). Whereas Binford had contended himself to dissecting one sheep and one caribou, now 'the effect of structural density on marmot skeletal part representation in archaeological sites' and 'a meat utility index for phocid seals' were being investigated, to name but

two articles of that period. The debate on skeletal element representation turned into a specialist and diversified field that risked losing touch with the original archaeological question.

In a different context, Longacre and Skibo (1994: xiv) noted that during ethnoarchaeological work it often ‘becomes clear that the question is much more complicated than originally thought’. And although this is probably the case with most empirical research, in the case of hunter-gatherer ethnoarchaeology there were Palaeolithic researchers ‘at home’ waiting for answers: ‘it is easy to see how a prehistorian could become frustrated with ethnoarchaeology, as seemingly simple questions are broken down into various parts that may seem far removed from the archaeological record’ (Longacre and Skibo 1994: xiv). Other scholars too have noted this. Gifford-Gonzalez (1991: 246) warned that ‘archaeology as a whole has moved increasingly deeply into specialization’; Hodder (1982b: 214) indicated the ‘massive fragmentation and compartmentalisation’ of research as one of the reasons for the decline of processualism.

Ethnoarchaeology became not only more technical but also more limited in its scope. Originally, actualistic studies and middle-range research had been formulated as the necessary step of understanding site formation before issues of general theory could be addressed. In the long run, however, it became an end in itself: ‘In their haste to put aside vacuous theorizing, middle-range theorists seem to have accepted the myth that their research can proceed in the absence of general theory,’ the result being ‘a widespread confusion regarding the nature of middle-range research’ (Bettinger 1987: 138). Even if studies of bone modification became quite successful through the use of actualistic research in identifying the causal agent responsible for a given trace, the wider behavioural implication remained more often than not unclear. Hominid-induced cutmarks could be recognized as such, the question whether they related to scavenging or hunting was still unanswered. Identifying the responsible causal agent was thus one problem, interpreting it in terms of behaviour quite another. Binford had been taking his logical premises to such extremes that he had driven himself into a corner where only very minimal statements could be made about very mechanical patterns.²³ Diane Gifford-Gonzalez (1991) convincingly argued that studies in zooarchaeology and bone taphonomy reached high levels of inferential confidence for the most functional and mechanistic links between traces and causes, but stayed underdeveloped for drawing broader behavioural and ecological conclusions. The insistence on finding unambiguous and uniformitarian causal linkages had turned the Binfordian middle-range research into an increasingly technical enterprise which showed great reluctance to more interpretative strategies. Such physicalist-reductionist and deterministic approaches continued to ‘hamper us at our next stage of research’, Gifford-Gonzalez argued (1991: 244):

23 When asked about this minimalism in the early 1980s during a visit in Sweden, Binford replied, true to the positivist belief in the growth of knowledge, that archaeology needed a few more centuries to reach reliable inferences about less mechanical patterns (K. Kristiansen, pers. comm.).

No matter how deterministic the relationship between the immediate causes of certain archaeological traces—and even their links to specific actors—are, when archaeologists seek to set these traces and actors in behavioral and ecological systems, the probabilistic nature of the operations of these systems preclude extending “if a, then b” deterministic statements into those realms. (Gifford-Gonzalez 1991: 241)

If archaeology wanted to be a truly behavioural science, more than such safe but sterile claims had to be made. Gifford-Gonzalez therefore reasserted ‘the importance of comprehensive, conjunctive analysis’ (246) and granted a place to ‘historical narratives’ (242) as satisfactory evolutionary explanations. Despite ‘an extraordinary successful 20-year phase of revealing ancient causes of bone modification’, she insisted that more interpretative work be undertaken: ‘bones themselves are not enough’ (246). O’Connell (1995), too, after listing hundreds of publications suggested that the quality of the analogies lagged behind with the amount of actualistic research conducted since the early 1980s. The wealth of ethnoarchaeological efforts in the study of site structure and faunal remains only seemed to indicate that there were no straightforward causal linkages between behavioural processes and material patterns. As a result, the current role of actualistic research simply became ‘the production of cautionary tales and conventional analogies’ (O’Connell 1995: 233). This meant an unabashed return to what ethnoarchaeology initially had tried to escape: the critical and inspirational purposes of analogy. Clearly, this was only a very minimal output for a field which required such high technical input and this imbalance started to undermine ethnoarchaeology’s *raison d’être*.

Another reason which contributed to the increasingly problematic position of hunter-gatherer ethnoarchaeology concerned the very polemical climate of scholarly debate. Binford, in particular, conducted a number of discussions with a vehemence of style rarely seen in scholarly discourse. Even if in the 1960s and 70s he had always been a provocative and very critical author who did not shun a rhetorical style, most, if not all, his papers in the 1980s consisted of fulminating attacks against the work of his colleagues. Whereas his collection of papers from the 1970s, a decade typified by his substantial contributions in theory and ethnoarchaeology, had been appropriately entitled *Working at Archaeology* (1983b), the one which assembled his work from the 1980s was even more aptly named *Debating Archaeology* (1989). After his disputes with Yellen and Isaac, Binford now crossed swords, among others, with Schiffer on the Pompeii premise, with Bunn on early hominid butchering at the Zinjanthropus site (Olduvai Gorge), with Freeman on Mousterian bone technology, with Sackett on the definition of style, with Hodder on the aims of archaeological research, and with Gould on about everything the latter had written since his negative review of *Nunamiut Ethnoarchaeology*. These attacks contained a set of fixed ingredients: Binford’s complaint that he had been misread, misunderstood, and misrepresented (illustrated by means of long quotes from his and others’ writings), the mercilessly chopping down of the other’s arguments, a long, poorly structured section which

sketched an alternative on the basis of his own ethnographic observations among the Nunamiut and the Alyawara incorporating multiple tables and graphs, and finally an abstract plea for how a ‘germane’ archaeology should be done. The tone of debate was often far from gentle. Bunn and Kroll could read that their study had ‘no intellectual anchor beyond an imagined picture of early hominid life’ (Binford in Bunn and Kroll 1986: 446). Freeman, who had denounced Binford’s ‘complex logical gymnastics’, was replied with the assertion that his argument rested on ‘many tenuous, and unexamined premises’, as well as ‘the use of nonfacts’ (Binford 1983c: 84–5). Hodder’s *Reading the Past*, ‘a little book with a little message being blown through a large horn with a loud noise’, was ‘packed with contradictions, misrepresentations, and distortions’: ‘Holy-moley, Hodder,’ Binford exclaimed, ‘you have just discovered science through Collingwood!’ (Binford 1988b: 875–6). Finally, Gould was told that he advocated ‘very outdated positions’, that he was ‘strangely combining philosophies’, that his writing was ‘opaque and hard to follow’, his presentation ‘frequently illogical and philosophically “innocent”’ and ‘his naivete’ ‘most interesting and unique’ (Binford 1985: 581, 584, 588), apart from the more general point that ‘his nonparticipation in science’ resulted from the fact that his arguments were ‘self-serving, misleading, confusing, and generally invalid’ (Binford 1989: 114, 117). Gould reproached Binford for frequently using the *argumentum ad hominem* (Gould 1985: 643), but Binford bluntly replied that Gould’s claims were often examples of the *argumentum ad ignorantiam* (Binford 1989: 111). The list could be easily expanded but there is no need to do so. Though such polemics render the task of the historian of science somewhat more juicy than it normally is, the systematic uttering of such devastating criticisms by the archaeologist who was commonly respected as the instigator of the New Archaeology left an ambivalent impact on the discipline. Gould believed that it would be unfortunate if such virulent polemics became ‘the role-model for the conduct of such debates’ (1985: 644):

The tone of Binford’s recent responses to my work is reminiscent of the robber barons of recent American business history who vigorously argued for unrestrained free enterprise and competition but who did their utmost to build monopolies. On the one hand, he reiterates the position that constant critical evaluation of theories and assumptions is needed [...] yet, when one is critical, even indirectly of Binford’s position, the response is that one is being “hostile,” “self-serving,” or “misleading,” that one is creating “misguided debates” and “distortions,” to mention but a few of the pejorative terms applied whenever there is disagreement over our paradigms. (Gould 1985: 643)

Of course, the role of critical debate, even hostile controversy, is often a very productive one in the development of science—Binford’s own work of the 1960s and 70s is a case in point—but in the context of a highly technical debate where evidence remained ambiguous (and tampering hard to detect) such polemical slaying tended to become counterproductive: at best, it gave the archaeological community the impression that the problems in ethnoarchaeology were far from being solved and remained utterly complex; at worst, it led to a weariness and

despondency about ethnoarchaeology because nothing, it seemed, could be done well. This applies especially to the field of faunal analysis with its multiple ‘bones of contention’ (Lewin 1997), but it was equally true of the controversial study of site structure before Whitelaw presented his synthesis, seen and approved by Binford. Whereas Binford’s earlier writings had opened up entirely new vistas for archaeological research, his overdefensive quarrels contributed to the isolation of hunter-gatherer ethnoarchaeology from more general theoretical themes. In the late 1970s ethnoarchaeology of foraging societies stood at the heart of processual debate; ten years later it had become a marginalized specialism where polemics were abundant but results inconclusive.

Anthropological doubts about hunter-gatherers

The historicist debate in hunter-gatherer anthropology also affected, albeit in an indirect way, the ethnoarchaeology of contemporary foragers. Ten years after the Man the Hunter conference, the French anthropologist Maurice Godelier convened a meeting in Paris to discuss the current state of hunter-gatherer research. It was the start of a profound revision of the Man the Hunter image, especially as it had been promulgated by the Kalahari research project of Richard Lee and his Harvard team: modern hunter-gatherers were no longer said to live in pure isolation, to subsist exclusively on foraging and to represent a timeless form of ecological adaptation (Bird-David 1988). Instead, the alternative image that emerged from this critique was that modern foragers had for centuries, if not millennia, been in contact with neighbouring pastoral and agricultural, and later even colonial and industrial societies and that these contacts had had a profound impact on the hunting-gathering way of life. The !Kung Bushmen could not be understood without taking into account their long-standing contacts with Bantu-pastoralists and their involvement with mining-industry in Namibia, Botswana and South-Africa. There were no longer hunters living strictly in a world of hunters and the idea of the pristine, pure, authentic, unaffected, unspoiled, and timeless forager was shown to be a Rousseau-like cliché that had crept into the functionalist discourse of 1960s and 70s cultural anthropology. Several anthropologists even questioned the validity of ‘hunter-gatherers’ as a distinct category and abandoned such generalizing concept altogether (Barnard 1983; Myers, 1988). This alternative interpretation of modern foragers was in the first place given in by a theoretical redefinition (though it was supported by new ethnohistorical evidence): against the functionalist anthropologies like neo-evolutionism (Service, Steward), cultural ecology (Lee), and cultural materialism (Harris) with their common focus on the social and economic structures of *individual* societies in their ecological surroundings, now in the context of structural-Marxist thought where world systems theory and core-periphery ideas were high on the agenda more attention was given to the historical importance of *intersocietal* contacts. In the same year of the Paris conference, Martin Wobst (1978) criticized the isolationist, parochial focus of hunter-gatherer research and Eric Wolf (1978) published his magnum opus: *Europe and the People without History*, both urging that so-called timeless, primitive societies be understood in historical terms of economic transactions and

political dominance. In more than one sense, the dissatisfaction with functionalism which was said to be 'ahistorical rather than antihistorical' (Schrire 1984b: 1) and the call for a fuller appreciation of history echoed Boas' critique against evolutionism and his insistence on the necessity of historical understanding in anthropology.

The historicist or revisionist debate became the key controversy of hunter-gatherer anthropology in the 1980s and early 1990s. At regular meetings anthropologists and ethnohistorians explored the role of contact, encapsulation, acculturation in the history of today's foraging societies (cf. Leacock and Lee 1982; Schrire 1984a; Ingold, Riches and Woodburn 1988). Journals like *Current Anthropology* and *Anthropology Today* published numerous articles on the debate (e.g. Testart 1988; Headland and Reid 1989; Solway and Lee 1990; Wilmsen and Denbow 1990; Lee and Guenther 1991; Ingold 1992; Stiles 1992; Shnirelman 1994) and the synopses of contemporary debates presented by the *Annual Review of Anthropology* came back to it several times (Barnard 1983; Bettinger 1987; Myers 1988). It became *bon ton* to despise the term 'pristine' and to stress historical interaction, although the extent to which acculturation had taken place was often harder to assess (Woodburn 1988). Throughout, the Kalahari debate remained the most contentious zone of disagreement: in 1992 Alan Barnard compiled a bibliographical essay on the theme, listing nearly 600 sources related to the debate (Barnard 1992).

Considering the zeal and energy devoted to this theme, the question must be asked as to how historical revisionism afflicted hunter-gatherer ethnoarchaeology. Time and again, historical revisionists claimed that, considering the profound impact of historical exchange and interaction, modern foragers could not be used as living stand-ins for the remote evolutionary past, a warning comparable to the Duke of Argyll's position in the 1860s. Still in 1989 two anthropologists found it necessary to criticize the idea that 'tribal peoples, and especially nomadic foragers, are often described as "fossilized" remnants of isolated late Paleolithic hunter-gatherers who have just emerged, through recent contact, into the 20th century' (Headland and Reid 1989: 43). Although at first sight this seemed to invalidate the ambition of ethnoarchaeology, it cannot be forgotten that ethnoarchaeology had incorporated this very idea into one of its axioms. It was precisely *because* nineteenth-century-like ethnographic projections were to be avoided that ethnoarchaeology sought alternative ways of looking at contemporary foragers; it was precisely *because* ethnoarchaeologists were aware of a discrepancy between source and target that concentration shifted from studying similarity to studying causality. Modern ethnoarchaeology was not justified 'to the extent that past forms persist in the present' (Ingold 1992: 793).

The historicist debate, therefore, was more endemic to social and cultural anthropology proper than it was to archaeology. At the Man the Hunter conference, archaeologists and anthropologists had been overtly communicating with each other; but at the Paris meeting ten years later (and other subsequent meetings), archaeologists were notably absent (Bender and Morris 1988: 6). It was Lee's (1979) cultural ecology of the !Kung that came under fire, not

Yellen's (1977) ethnoarchaeology of them. In fact, the names of Yellen, Binford, Gould, O'Connell and others hardly, if ever, popped up in the revisionist papers. No matter how legitimate historical revisionism in anthropology was, it did not directly apply to hunter-gatherer ethnoarchaeology because individuals like Binford and O'Connell had rarely needed the assumption that they were working in pristine foraging contexts: at least in terms of material culture, the presence of imported items like rifles, sardine cans, pop cans, sheets of corrugated iron and second-hand cars was acknowledged in text, tables and photographs. And even if there lingered a tendency to describe Nunamiat and Alyawara subsistence and social system in terms of self-sufficient, autarkic foraging adaptations (Binford's (1980) ideal-typical distinction between collectors and foragers was largely a universalized reification of the historically contingent subsistence strategies observed among the !Kung and the Nunamiat), the historicist critique did not fundamentally alter their perception because in general ethnoarchaeologists were not looking for living relics of past socio-economic forms but were simply interested in the modern workings of a cultural system, regardless of whether it was pristine. To the revisionist critique formulated by Headland and Reid, the archaeological reply in *Current Anthropology* was: 'While the ethnographic record is in itself clearly not an archive of earlier evolutionary forms, it can be used as an arena within which to investigate organizational relationships among sets of variables relevant to the formulation of models for prehistoric situations' (Hutterer in Headland and Reid 1989: 57).

If the historicist debate affected ethnoarchaeology, it happened at best indirectly by sweeping away the anthropological ground from which processual archaeology had sprung. Had hunter-gatherer ethnoarchaeology once been flanked and buttressed by neo-evolutionist and functionalist anthropology, the damning critique against the latter turned ethnoarchaeology into an orphaned province within archaeology. Ethnoarchaeological work with the !Kung became much less prestigious the moment Lee's cultural ecology was despised. So when the historicist debate did not aim its arrows at ethnoarchaeology, it nonetheless contributed to the process of isolation that was already set in motion by ethnoarchaeology's internal difficulties. Too technical, too restrictive and too polemical, hunter-gatherer ethnoarchaeology was now further deprived of the patronage of anthropology from which it had so long benefited.

Contextual ethnoarchaeology²⁴

Granted that structural-Marxist anthropology contributed only indirectly to the decline of hunter-gatherer ethnoarchaeology, it affected the archaeology much more immediately by inspiring a number of mostly British scholars in the early 1980s who turned its theoretical bases into a full-blown critique of processual archaeology. A Marxist interest had been present in North-American scholarship for a long time, particularly in the work of individuals like Bruce Trigger, Thomas Patterson and Mark Leone, but lacking a coordinated effort, it did never overthrow

24 This and the following section were reworked into a separate article (Van Reybrouck 2000).

the deeply-entrenched functionalist orthodoxy. In Britain, however, processual archaeology had always taken a somewhat less extreme form than on the other side of the Atlantic.²⁵ David Clarke's *Analytical Archaeology*, the key text for the British pendant of the New Archaeology, systematized rather than criticized the workings of culture-historical archaeology. Colin Renfrew, though clearly inspired by American developments, avoided ecological determinism through his emphasis on issues of social archaeology in later British prehistory. The adaptationist, hyperfunctionalist, and nomothetic strands of processual archaeology were attenuated in the British context where the New Archaeology's largest impact was made in the development of explicit theory (see Clarke's work on systems theory and model building) and better methodology (see Renfrew's work on radiocarbon dating and Clarke's work on spatial analysis). Taken together with David Clarke's early death and the longevity and continuing influence of scholars of the older generation (Grahame Clark, Glyn Daniel, Stuart Piggott), the British version of processualism was considerably less dogmatic and less consolidated than its overseas variant.²⁶ A younger generation of researchers could, therefore, start to develop alternative perspectives by associating themselves with the previous tradition of humanistic archaeological scholarship. Hodder said: 'Writers such as Childe, Clark, Daniel and Piggott placed a similar emphasis on archaeology as an historical discipline, they eschewed cross-cultural laws, and they saw material items as being structured by more than functional necessities' (Hodder 1982d: 11).

Another influential source of inspiration came from London-based archaeologists like Barbara Bender and Mike Rowlands whose contacts with anthropologists had already given rise to an important interest in structural-Marxism. Unlike the North-American Marxists, the efforts of these British critics of processualism were well coordinated: they were centred in one place (Cambridge, and to a lesser extent London), they had a forum for discussion (the series *New Directions in Archaeology* at Cambridge University Press), and they had a generally acknowledged spokesperson (Ian Hodder).²⁷ Hodder, one of Clarke's former students, had originally undertaken research in model-building, spatial analysis and computer applications but started to criticize systems theory and the dominant

25 British processual archaeology also converged with the older functionalist approach. Grahame Clark's student Eric Higgs promulgated the study of ecological archaeology to an entire generation of students who had also sympathy with processual ideas. The New Archaeology's stress on ecology was in fact already responded to in the British tradition.

26 The relative flexibility of British processualism is also clear from the fact that some of its initial adherents such as Richard Bradley could fairly easily integrate elements from the contextual and post-processual programme. Indeed, the emphasis on the social subsystem eased the acceptance of an alternative perspective of social, symbolic and ideational archaeology. Renfrew's recent cognitive-processual archaeology, too, resulted from combining processual and post-processual strands, whereas Hodder originally started as a processualist working on spatial archaeology and computer simulations.

27 Most, if not all, post-processual volumes were published by Cambridge University Press. Between 1982 and 1987, the series *New Directions in Archaeology* published five edited volumes, all of which were essential in the development and dissemination of structural-Marxist and post-processual thought: *Symbolic and Structural Archaeology* (Hodder 1982c), *Ideology, Power and Prehistory* (Miller and Tilley 1984), *Marxist Perspectives in Archaeology* (Spriggs 1984), *Archaeology as Long-Term History* (Hodder 1987a), and *The Archaeology of Contextual Meanings* (Hodder 1987b).

functionalism in a series of books and articles from the late 1970s on. His article ‘Theoretical archaeology: a reactionary view’ (Hodder 1982d) served as a similar call to arms as Binford’s ‘Archaeology as anthropology’ written exactly twenty years earlier: Hodder was soon supported by authors like Daniel Miller, Christopher Tilley, Mike Parker Pearson and Henrietta Moore. Originally nameless, this counter-movement came to be labelled as ‘symbolic and structural archaeology’, later as ‘contextual archaeology’ and finally as ‘post-processual archaeology’, a name vague enough to shelter the variety of family-related approaches which had emerged since the incipience of the anti-processual critique.

Theoretical inspirations

Structuralism, structural-Marxism, structuration theory, neo-Marxism, critical theory, the sources of inspiration for this alternative form of archaeology were rather numerous. Whereas initially such external borrowing from sociology, anthropology and philosophy was beneficial to find a way out of the functionalist deadlock, in later phases it became downright fashionable to optimally forage the library bookshelves for social theory, literary criticism and continental philosophy that substantiated, or at least flanked, the archaeological claim (Van Reybrouck 1996; Murray 1996). During the earliest phase of contextual thought, two ranges of perspectives from the social sciences were particularly influential: structuralism and Marxism which, albeit both in a rather mitigated fashion, gave rise to two basic tenets, i.e. the ‘meaningful constitution’ and the ‘active role’ of material culture. From structuralism came the interest in mental and ideational aspects of societies and, more particularly, the idea that a society’s cultural norms and values are not just reflected in thought and language but also in settlement layout, house architecture, cooking, bodily decoration, and so on. Material culture was, therefore, not simply an expression of functionalist necessities and extra-somatic adaptations but could be seen as ‘meaningfully constituted’, a point repeatedly stressed by Hodder (1982b: 218; 1982d: 13). Close to structuralist linguistics (De Saussure) and anthropology (Lévi-Strauss, Turner, Douglas), it was believed that meaning of a sign emerged not so much by external reference to a semantic signified, but by internal contrast and opposition to others signifiers. Geometric decorations on a pot were not to be translated in what they represented from the real world, but in how they interacted with decorations on other material items. Dirt was not an intrinsic property of an object, but determined by its structural relations in human categorization. Hodder, however, was wary of the rigidity of Lévi-Straussian structuralism as it left little room for historical change and individual agency. He therefore embraced the nascent field of structuration theory, a modified form of structuralism advocated by Bourdieu and Giddens who argued that structures were not just silently and obediently reproduced by social actors, but simultaneously produced and modified through individual action. Hodder’s structuralist work was not just about symbols, but about ‘symbols in action’ (1982b). This greater emphasis on human agency, individual knowledgeability, and personal negotiation enabled to make a bridge to neo-Marxist thought.

Through the work of French anthropologists, particularly Godelier, Marxist thought with its traditional focus on infrastructural themes like economy, technology and social relations of production had been enriched to incorporate a fuller understanding of power and ideology. Ideology, neo-Marxists argued, could not just be seen as a superstructural mask for the underlying social realities but was, certainly in pre-capitalist societies, closely entwined with the mode of production; more than blatant false consciousness, ideology and power had to be understood as inherent forces operative in all forms of social action (an idea that was further supported by the historiographical work of Foucault). According to this view, material culture did not just belong to the forces of the socio-economic infrastructure but played a role in ideological and power-related strategies as well. At a nondiscursive and nonverbal level, material culture served to legitimate and manipulate social reality. Although Hodder aligned himself more closely to structuralism than to neo-Marxism, archaeologists like Miller and Parker Pearson further elaborated his important notion of ‘the active role of material culture’ (Hodder 1982b: 228). This catchphrase betrayed a crucial rupture with preceding archaeological thought. Material culture was no longer simply a *mirror* of social action, but equally an *instrument* of it, that is, like language, it could be used to communicate and negotiate, to challenge and change the given social order. Material culture could be appropriated by individuals in their strategies of self-representation and social action. ‘Material culture patterning transforms structurally rather than reflects behaviourally social relations’ (Hodder 1982b: 218). Such conceptualization of material culture was diametrically opposed to the reflectionist theory of processual archaeology: material statics had now an inherent dynamic potential.

Despite all later additions and external borrowings, it was the structural-Marxist legacy which provided the two pillars of subsequent post-processual thought. It resulted in a comprehensively new view of material culture as a realm of sociocultural practice imbued with ideational and ideological meaning that could be drawn upon in social action. True to Spinoza’s dictum *omnis determinatio negatio est*, contextual archaeology thus defined itself in opposition to the axioms of processualism, just like the New Archaeology before had been a counter-stand against culture-historical archaeology. Though the range of ideas was broad, textbooks and reviews generally enumerate following contrasts with the processual paradigm as the movement’s basic tenets (Renfrew and Bahn 1991; Dark 1995): its philosophy of science was less realist, less objectivist, less positivist but more inclined towards idealism, subjectivism and constructivism; in terms of explanation, it criticized logical proof in favour of historical understanding; it was more affiliated with history than with functionalist anthropology (although at first it derived many of its ideas from structural-Marxist anthropology); substantially, it urged to take the upper rings of Hawkes’ ladder of inference seriously, rather than to reduce them to the lower ones as had too often been the case with processual work; it emphasized ideational aspects of material culture besides functional ones; it urged that material culture was meaningfully constituted in that it reflected norms and values held by the members of a society; it stressed the role of individual agency and freedom against the view of a collectivist, adaptationist,

ecologically-steered form of human action; it argued for an active role of material culture rather than it being a passive reflection of social reality; in terms of archaeological writing, it preferred literary style over scientific jargon, narrative prose over sterile logic, metaphor over method.

A new ethnoarchaeological enthusiasm

How was the attitude towards ethnoarchaeology? Considering the drastic traposition against all processual premises, one would expect that a field which was once the hallmark of archaeological functionalism be immediately repudiated by the young critics. Indeed, reviews of the theoretical debate of the last decades often keep silent about such a thing like ‘contextual ethnoarchaeology’ (Renfrew and Bahn 1991; Dark 1995; Shanks and Hodder 1995), giving the impression that the whole field was rejected in the early 1980s along with other processual themes like formation processes, optimal foraging theory, site catchment analysis, and so on.²⁸ Nothing could be less true, though. Even if in more recent years post-processual writers have dispelled the term ‘ethnoarchaeology’ from their vocabulary, this obliterates the quintessential role ethnoarchaeology has played in the early phases of contextual archaeology. Consider the corpus of foundational texts, monographs and edited volumes alike, published in the first five years of contextual thought: Hodder’s three 1982 volumes *The Present Past* (1982a), *Symbols in Action* (1982b), and *Symbolic and Structural Archaeology* (1982c), Miller and Tilley’s *Ideology, Power and Prehistory* (1984), Miller’s *Artifacts as Categories* (1985), Moore’s *Space, Text and Gender* (1986), Hodder’s *Reading the Past* (1986), his edited volumes *Archaeology as Long-Term History* (1987a) and *The Archaeology of Contextual Meanings* (1987b), and Shanks and Tilley’s ‘black and red’ volumes *Re-Constructing Archaeology* (1987a) and *Social Theory and Archaeology* (1987b). Now, all these volumes, as is well known, incorporated to a greater or lesser extent a critique of the methods and principles of processual archaeology. Surprisingly however, and this is a point which has often been overlooked, ethnoarchaeology was never under fire. On the contrary, all contextual archaeologists had at one stage or another been involved with ethnoarchaeological research: Hodder studied decorative symbolism on pots, stools and persons among the Baringo pastoralists in Kenya (1982b; 1986: 107-20); Miller investigated the social and technological categories underlying Dangwara pottery in India (Miller 1985); Moore worked on settlement layout and refuse disposal with the Marakwet in Kenya (Moore 1986). Furthermore, Hodder (1982a: 215-6) drew attention to the material culture items appropriated by punks, Shanks and Tilley (1987a: 172-240) studied differences in design between Swedish and British beer cans and Miller (1984) analysed contemporary suburban architecture in Britain. The industrialized world was thus equally considered a promising field for material culture studies. On top of that, the volumes edited by Hodder (1982c, 1987a and 1987b) and

28 The term ‘contextual ethnoarchaeology’ never gained a wide currency after it was originally suggested by Hodder (1982d: 13). I will use it to designate the ethnoarchaeological studies conducted by Hodder and his students in early and mid-1980s.

Miller and Tilley (1984) all contained parts devoted to studies in ethnoarchaeology, ethnohistory and modern material culture. An archaeological approach to the present, then, was a very popular theme within early contextual work. In fact, in Britain a genuine interest for ethnoarchaeology was only launched with the rise of contextual archaeology. Unlike the close affinity between ethnoarchaeology and processualism in North-America during the 1970s, British processualists had only rarely shown much interest in actively undertaken ethnoarchaeological fieldwork.²⁹ The importance of ethnoarchaeology was only appreciated in the context of an emerging contextual approach; Hodder wrote that because so little was known about the place of material culture in daily practice, the role of ideology, the discursive and non-discursive dimensions of material symbolization, 'the main response to the new questions has naturally been to turn to ethnoarchaeology' (1982d: 14). If British ethnoarchaeology was largely contextual, contextual archaeology was also largely ethnoarchaeological. The first major monographs resulting from this theoretical upheaval were all ethnoarchaeological studies (Hodder 1982b; Miller 1985; Moore 1986); it would take several additional years before the first substantial book-length treatment on an archaeological problem would appear, which was arguably Hodder's *Domestication of Europe* from 1990.

Though ethnoarchaeology was far from being abandoned by contextual archaeologists, its emphases were quite different from the work conducted in a processual framework. First, the emphasis was no longer on the few surviving hunter-gatherers in arid and arctic environments, but on societies of pastoralists and agriculturalists, often in East-Africa where metallurgy was known, which showed varying degrees of exposition to the Western world.³⁰ This change of subject matter was closely paralleled by a shift from a research interest in Palaeolithic archaeology to one in more recent phases of European prehistory (Neolithic, Bronze Age, Iron Age). Concomitantly, the attention to archaeological site formation processes in terms of site structure and faunal assemblages was replaced by an emphasis on the active role and symbolism of pottery, vernacular architecture, and refuse disposal. More than developing 'cross-cultural predictive laws or generalisations [...] for these mechanical constraints on human behaviour' alone, 'the role of ethnoarchaeology must also be to define the relevant cultural context for social and ecological behaviour' (Hodder 1982d: 5). There was also a change in methodology: whereas processual ethnoarchaeology could in principle suffice with observing human behaviour and measuring variables often considered curious and trivial by the people under study, long-term participation, inside knowledge and

29 If there had been an interest in ethnography, it never entailed ethnoarchaeological fieldwork. David Clarke, as indicated above, used the results from Bantu ethnography to question the archaeological culture concept. Colin Renfrew worked with social-anthropological notions like a-cephalous societies, 'big men', bands and tribes to explain late-prehistoric Britain, but this did not entail ethnoarchaeological expeditions to Melanesia and Polynesia (where such concepts had been developed by anthropologists like Sahlins).

30 I am fully aware of the existence of a range of processual ethnoarchaeology that was not focused on hunter-gatherers like the work on rural settlements in Iran (Kramer 1982; Watson 1979b) and ceramic practices in the Philippines (Longacre 1991; Longacre and Skibo 1994). However, hunter-gatherer studies belonged to the realm where ethnoarchaeology was processualism *par excellence*.

historical analysis were deemed indispensable by the contextual archaeologists (Hodder 1986: 108). This implied a shift from the outside to the inside, from an etic to an emic perspective, from observing to participating, from explaining site formation to interpreting material culture. Yet all these differences can only be understood if one appreciates the radically different function contextual archaeologists accorded to studying the contemporary world. Indeed, ethnoarchaeology no longer served to unravel the relevant causes that had formed the archaeological record, but to indicate general principles regarding the role of material culture in social practice. Processual ethnoarchaeology, with its emphasis on middle-range research and actualistic studies, had been largely methodological; contextual ethnoarchaeology was largely theoretical. Whereas processual ethnoarchaeology had increasingly shunned issues of general theory, entrenching itself in all sorts of technicalities, contextual archaeology used ethnographic studies to explore, develop, and disseminate a range of alternative perspectives on material culture.

This can best be illustrated with two examples. Hodder's (1982b) classical research in the Baringo district, Kenya, originally started with the hope to see what archaeological cultures could have meant in reality, comparable to how his mentor David Clarke had used Bantu ethnography to argue that archaeological cultures do not represent ethnic identities.³¹ Hodder soon found out that ethnic entities did not necessarily correlate with a recurrent set of artefacts nor that increased interaction between two ethnic groups led to increased stylistic similarity. Quite on the contrary, he noted that intercultural interaction might rather fortify stylistic differences; in contact zones, material culture was apparently appropriated to affirm ethnic identity and affiliation (figure 13). This awareness of how individuals could negotiate their social positions through material strategies led Hodder to study other boundaries which crosscut strictly ethnic ones. He observed that material culture played an equally important role in policing age and sex boundaries when young men used age-related spears and women specific forms of calabashes to challenge the authority of elder men. Material culture patterning was more than a 'predictable reflection of human behaviour'; it entailed 'ideological manipulation of material items in social and ecological strategies' (Hodder 1982b: 11, 229). Yet more than an instrument of identity bargaining and manipulation, Hodder argued that all aspects of material culture and related social practice were manifestations of a same underlying, symbolic scheme: a cultural logic based on binary oppositions like pure versus dirt, and insider versus outsider (which betrays Hodder's indebtedness to the British school of structuralism, in particular the work of Mary Douglas and Victor Turner). Active role of material culture and meaningful constitution of it, ethnoarchaeology was instrumental in laying the bases of contextual thought. For Hodder ethnoarchaeology was meant to show

31 At that point in his intellectual development Hodder was still strongly inspired by David Clarke: his ethnoarchaeological work initially started to criticize the basic premise of Childean culture-historical archaeology (i.e. the assumption that archaeological cultures represented ethnic identities). Only after Clarke's death did it develop into a critique of the reflectionist assumption of processual archaeology (Hodder 1981).

'how structures of meaning relate to practice — how symbol sets are negotiated and manipulated in social action' (1982b: 214).

The same holds true for Mike Parker Pearson's (1982) study of mortuary behaviour, even if it dealt with the industrialized world. He argued that mortuary practices in Victorian and modern Britain had to be understood by the interplay of ideational structures in terms of purity and danger with ideological strategies of social display. Whereas in the nineteenth century, elaborate tomb architecture and other forms of wealth display served as forms of social advertisement, the sobriety of twentieth-century graves and the growing popularity of cremation reflected the increasing impact of hygiene, as dictated by science and medicine, on social practice. Not only did the structural logic undergo a profound transformation and redefinition, the role of material culture as identity-marker did too. Parker Pearson's study was exemplary of the early contextual archaeology in that it used a contemporary context, even from an urban setting, to develop and communicate novel theoretical insights about the interplay of structuralist and Marxist principles in material culture practice.

The problematic place of analogy

Clearly, studying Kenyan calabash design and Victorian tomb architecture is quite another form of ethnoarchaeology than quantifying the number of broken reindeer metatarsals at an Inuit site. Far away from the aspirations of middle-range theory, for Hodder and his students ethnoarchaeology was essential in discovering, developing and disseminating the basic tenets of contextual archaeology as an alternative to mainstream processual thought. Just like people in the Baringo district used material culture to express ethnic identity and difference, contextual archaeologists used material culture studies to express divergence from functionalism and affiliation with structural-Marxism. Predictably, the role of analogy was a rather different one. Hodder originally embraced Wylie's measures for improving the argument by analogy: *The Present Past*, his programmatic exposé on ethnoarchaeology (1982a: 16–27), reiterated her call for relational analogies that are based on relevant similarities across a number of different sources instead of listing formal similarities in the belief that testing or falsification will be enough. However, Hodder never put this call into practice.³² The awareness of causality, Wylie asked for, was immediately invalidated by Hodder when he said that 'there can be no simple functional links' in the source of the analogy (1982a: 24). Wylie asked for causes, Hodder said they could not be found: 'We see that there can be no universal cultural relationship between statics and dynamics, because the historically

32 Later, in *Reading the Past*, Hodder (1986: 148–9) suggested that the argument by analogy which assesses the relevance of similarities and differences was 'simply another instance of the general [contextual] approach already outlined': the interpretation of meaning through the contextual method he advocated would also have worked from determining the similarities and differences between archaeological objects. It seems, however, that Hodder has been misled here by the identity of the terms 'similarity' and 'difference' used, because the analogical method, unlike the contextual method, required an understanding of causality and an assumption of uniformitarianism, issues that were not covered by the contextual approach.

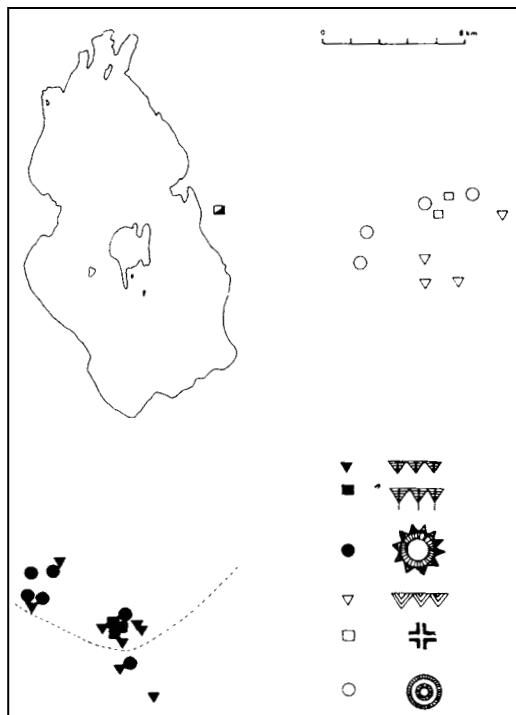


Figure 13. Hodder used ethnoarchaeology to investigate the active role of material culture. Calabash designs in the Baringo area of Kenya increased with cultural contact between different ethnic groups. Interaction did not result in stylistic homogenization, but in differentiation. Material culture was more than a reflection of the social order (Hodder 1982b: figure 37)

contextual structuring principles intervene' (Hodder 1986, 120). And even if such particular historical contexts could in principle still supply relational analogies, the idea of 'causal certainty' was replaced by a vaguer notion of 'potential connection' (Holtorf 2000).

Despite Hodder's initial wish to reconcile an ideographic approach with a reliance on analogy (1982d: 13), the particularist emphasis on attitudes, concepts, values and ideas undermined the possibility of drawing relational analogies in the sense Wylie suggested. Not only was the uniformitarian assumption between source and target analogue unsubstantiated, the internal links within the source were never unambiguous or sufficiently causal to support an analogy. Even if the active role of material culture could reverse the causality arrow because the archaeological statics were now (to some extent at least) the causing agent of cultural dynamics, this did not ease the drawing of analogical inferences.³³ In general, however, such inverse causality strengthens the analogy (Wylie 1982): because the antecedent (the determining cause of behaviour) is known for both the source and target (present and prehistoric material culture), the problem of equifinality

³³ Binford (1989: 30) rightly observed that in contextual and post-processual thought ‘there is no difference between the static and dynamic worlds, contrary to what most who have seriously addressed the problem of formation processes for the archaeological record have clearly suggested.’

is minimal (the trouble that a same pattern can have different causes). It is much easier to reason from cause to consequence than vice versa. Yet this was not the case with contextual ethnoarchaeology. The principle of the active role of material culture was not so much about inverted causality as it was about the absence of clear-cut causality. It did not state that material culture steered or dictated social action, but that it could be used as an instrument of social negotiation. It was not about material culture as the causing agent, but about a role bestowed upon it by human actors. Material culture did not act out of itself (apart from the consequences unintended by the one who used material culture as non-verbal discourse), it played a role written and directed by knowledgeable human beings—and these were still quite absent from the archaeological record. With the decline of the reflectionist view of material culture, causality evaporated from the ethnoarchaeological discussion: as a mirror, material culture could reflect only one reality; as an instrument, however, it could play very different tunes.

Now that understanding causality was no longer within the grasp, contextual archaeologists often slid back into an older and weaker form of analogical reasoning, i.e. the use of observed but unexplained cross-cultural similarities in the source as a basis for making predictions about the past. The method essentially went back to the nomothetic endeavours of Schiffer c.s. in the 1970s and it is perhaps because of this processual smack that contextual ethnoarchaeologists injected it always tacitly. For example, in studying Neolithic long houses, Hodder reasoned from the cross-cultural hypothesis that ‘in small-scale lineage-based societies in which the major concern is to increase labour power, the control of women by men and the negotiation of position by women will become the dominant feature of social relations and will often involve cultural elaboration of the domestic sphere’ (1984: 61). Although he warned against placing ‘much reliance on these ethnographic analogies without a careful consideration of the contexts involved’, he found ‘the widespread relationship’ between the varying elaboration of houses and the varying position of women to be ‘suggestive’ (62); indeed, it even formed the backbone of his entire argument. It was a downright cross-cultural analogy: the observed similarity (‘elaborated houses’) led to a predicted similarity (‘female negotiation’) on the basis of a cross-cultural linkage between the two in the present. Shanks and Tilley, though generally wary of ‘the sledge hammer of cross-cultural generalization’ (1987a: 95), relied on such an argument in their study of southern Swedish middle Neolithic ceramics when they wrote that ‘in small-scale “traditional” societies in which artistic production is highly ritualized, little room is left for individual expression or innovation in form or the introduction of new or radical content’ (1987b: 163). Contrary to what they pretended, they were *not* ‘eschewing cross-cultural generalization with its resultant problems of lack of explanation of specific features of material culture’ (1987a: 113). On top of that, the structural-Marxist underpinnings encouraged a cross-cultural perspective: Hodder insisted that even if every cultural context was unique, ‘universal principles of meaning’ existed (1986: 127). Contextual archaeology had to reconcile the specific with the general: ‘each particular historical context must be studied as a unique combination of general principles of meaning and symbolism,

negotiated and manipulated in specific ways' (Hodder 1982b: 218). In practice this came down to covertly using cross-cultural generalization while overtly despising them.

The discrepancy between the finicky scrutiny of processual ethnoarchaeologists in the 1980s and the sweeping claims of the contextual scholars was so large that discussion between them remained absent. Binford's (1989: 27-71) predictable and multifarious attacks against the post-processualists did hardly, if ever, touch upon the problem of ethnoarchaeology and analogical reasoning. Hodder was one of the rare authors to address the problem more explicitly. In *Reading the Past* (1986: 107-20), he questioned the validity of processual actualistic research:

While the idea of Middle Range Theory in relation to physical processes (e.g. decay of C¹⁴) is feasible, it is difficult to see how there can ever be universal laws of cultural process which are independent of one's higher-level cultural theories.
(Hodder 1986: 107)

Considering the uniqueness of cultural norms and values, Hodder suggested that ethnoarchaeologists should incorporate the methods of social and cultural anthropology rather than becoming a positivist discipline of physical processes and mechanical causation. However, the question then emerged if there was still any difference between such participatory ethnoarchaeology on the one hand and traditional anthropology on the other. 'Should ethnoarchaeology not disappear, to be replaced by or integrated with the anthropology of material culture and social change?' he wondered (108). Indeed, anthropology was already witnessing an increased interest in material culture so that ethnoarchaeology had perhaps only been a 'stop-gap' or a 'period piece' invented by archaeologists at a time when anthropology showed a lack of concern for material culture (108). According to Hodder, ethnoarchaeological research might therefore ultimately be 'associated with the non-contextual, cross-cultural trends in the archaeological science of the sixties and seventies' (108). He believed that 'whatever the long-term future', ethnoarchaeology would still 'retain a role in the immediate future' of the discipline (108). Hodder was right in doubting the longevity of ethnoarchaeology, but that its expiry date would come so rapidly was even beyond his most realistic expectations.

Post-processual archaeology

Hodder's trust in the immediate future notwithstanding, the decline of ethnoarchaeology during the 1980s is probably nowhere better illustrated than in his own list of publications. In 1982, the birth year of contextual archaeology, Hodder published the results of his ethnoarchaeological fieldwork in Kenya in *Symbols in Action*. At the very end of this book, there was a case study on settlements and tombs in Neolithic Orkney which drew on the ethnoarchaeological insights of symbolic codification. Hodder stressed that the development of a contextual archaeology depended 'to a large extent on the further expansion of ethnoarchaeological investigations' (1982b: 229). His 1984 study of Neolithic long houses in

central Europe suggested that their organization of space was linked to female negotiations of position through the internal arrangement of domestic space as observed in East Africa and elsewhere (Hodder 1984). This was a direct transposition of ethnoarchaeological principles to prehistoric contexts. In subsequent years, Hodder maintained a decided interest in ethnographic, ethnohistorical and ethnoarchaeological studies (see his edited volumes from 1982c, 1987a and 1987b), but the feedback to prehistory was increasingly bracketed. His 1986 plea to draw ethnoarchaeology closer to social anthropology ‘also militate[d] against a “materialist”, “archaeological” ethnoarchaeology’ (1986: 107). *Reading the Past*, therefore, marked the transition from contextual ethnoarchaeology to post-processual archaeology, that is, the transition from ethnoarchaeological enthusiasm to epistemological scepticism. When in 1990 his *Domestication of Europe* appeared, Hodder avoided any overt influence from previous ethnoarchaeology: ‘I do not think that I consciously used the Nuba as an analogy,’ he said when discussing the architectural ornamentation of Çatal Hüyük (1990: 5). An archaeological ethnoarchaeology, i.e. one that is directly relevant to the study of the past, thus disappeared from sight. To put it to the proof: in *Theory and Practice in Archaeology*, Hodder’s collection of articles from the 1980s and early 1990s (Hodder 1992), the term ‘ethnoarchaeology’ virtually disappeared from his writing during the second half of the 1980s. The rare cases it is mentioned after 1986 invariably belong to contexts of critique and scepticism.

This gradual disappearance of ethnoarchaeology in Hodder’s writings was not necessarily the cause for a wider phenomenon but quite symptomatic of it. Indeed, the pattern observed was not restricted to the work of Ian Hodder alone. Following his *Domestication*, the early 1990s testified to the publication of monographs like Julian Thomas’ *Rethinking the Neolithic* (1991) and John Barrett’s *Fragments from Antiquity* (1994), works which clearly associated themselves with the post-processual approach, which tried to shed new light on certain aspects of later European and British prehistory, but which invariably eschewed the use of ethnographic analogies and ethnoarchaeological conclusions. Though this omission, or rather, exclusion is rarely defended as a deliberate choice, the consistency of the pattern makes it more than just accidental. Indeed, in none of these volumes does the word ‘ethnoarchaeology’ occur in the index. If a book’s index can be seen as the shortest introduction to its intellectual idiom and key concepts, the total absence of an ‘ethnoarchaeology’ entry reveals a marked change in the conceptual apparatus of post-processual scholarship. Though this omission, or rather, exclusion is rarely defended as a deliberate choice, the consistency of the pattern makes it more than accidental. Preucel and Hodder’s massive reader *Contemporary Theory in Archaeology* (1996) was even entirely silent on ethnoarchaeology.

Rhetoric, theories, and politics

Three reasons must be taken into account to explain the shift from a contextual ethnoarchaeology to a post-processual archaeology: a rhetorical, a theoretical, and a political one. Firstly, once the axioms of contextual archaeology had been abundantly made clear through the studies of the living world, there was no longer a

need to draw upon such ethnoarchaeological and modern material culture studies. On the contrary, it was rhetorically injudicious to continue that indirect strategy of theoretical exploration: those already convinced by the new approach did not need any further examples from the present, while those still sceptical wanted to see how such an alternative perspective might improve an understanding of the past itself. Hodder opened *The Archaeology of Contextual Meanings* with often heard critiques like ‘The approach cannot be applied’, ‘You can do such studies in the present but not in the past’ which he replied fiercely with the ambitious promise: ‘The aim in this volume is to show that “it can be done”’ (Hodder 1987b: vii). Similarly, Tilley’s first lines in the preface of his edited volume *Interpretative Archaeology* read:

One of a legion of criticisms directed against the recent emergence of a ‘post-processual’ archaeology has been that theoretical exposition dominates and it lacks many clearly worked-out examples tackling archaeological data. Those given often discuss contemporary rather than prehistoric material culture. The purpose of this book is to address that ‘failure’ and provide a sense of excitement of carrying out archaeological research in a new way [...]. The studies in it consist of detailed explorations of the past. (Tilley 1993: ix)

To be a truly cogent alternative, compelling archaeological case-studies were needed instead of further ethnographic examples.

Secondly, the late 1980s had brought about a theoretical climate which, although heavily indebted to preceding contextual thought, implied fundamental changes in the appreciation of ethnoarchaeology. Put schematically, if contextual archaeology had borrowed many of its principles from structuralism and neo-Marxism, post-processual archaeology found inspiration in hermeneutics and post-structuralism. Whereas the former were traditions of anthropological thought with an inherent cross-cultural perspective, the latter came from trends in philosophy and literary criticism which emphasized alterity, difference and uniqueness. Contextual archaeologists stretched structural-Marxist anthropology to include ethnographic fieldwork with a consideration for material culture; post-processualists focused more on the ‘reading’ of prehistoric ‘texts’ whereby interpretation proceeded without recourse to present parallels. Structural-Marxist explanation implied reference to general principles like class struggle, social display, binary opposition and codic transformation; post-structural and hermeneutic approaches, on the other hand, preferred ongoing interpretation, creative empathy, individual understanding, and open-endedness. Since ethnoarchaeology required a minimum of cross-cultural comparison and a certain belief in the possibility of generalization, its place became increasingly problematic in post-processual discourse.³⁴ The extreme particularism which resulted from this new theoretical course limited the role of ethnoarchaeology: this could only show *that* material

³⁴ An interesting point for a history of archaeological publishing is that the transition from contextual to post-processual archaeology was accompanied by a shift from the somewhat austere bastion of academic publishing, Cambridge University Press, to the more fashionable, post-modern fonds of Routledge (and to a lesser extent Blackwell).

culture played an active role, not *which* one; *that* it was socially constructed, not *how* exactly in particular cases. Detailed contexts were thought to be so much determined by particularistic contexts and historical idiosyncrasies that analogy was no longer of any practical use.

To the names of Lévi-Strauss, Bourdieu, Giddens, Althusser and Godelier were now added the ones of Ricoeur, Gadamer, Collingwood, Derrida, Barthes, and Foucault. Hodder's *Reading the Past* had already advocated a return to the hermeneutic philosophy of Collingwood (1986: 95-101), Moore's *Space, Text and Gender* (1986) had relied in part on the critical hermeneutics of Ricoeur (1986: 80-4; cf. Moore 1990) but it was in particular Shanks and Tilley's *Re-Constructing Archaeology*, drawing upon the work of Gadamer, which steered the field into a more hermeneutic course (1987a: 103-110). Hermeneutics, or the art of understanding, investigated what happened in the process of interpretation, in particular of historical texts, but by extension of all forms of human communication, from paintings and operas to dresses and gestures. One of its central insights, suggested by Gadamer and explored by Ricoeur, was that meaning was not just passively read *off* a text, but actively read *into* it. Meaning did not reside inside the text but emerged at the interplay of it with the interpreter. This opened up numerous possibilities for the interpretation of so-called mute material culture; understanding was no longer a process of unprejudiced decoding but involved a dialogue between the investigator's frame of reference and the constraining evidence at hand. (Shanks and Tilley's writing, in fact, was further complicated because it drew as much on hermeneutics as on critical theory, the one being a phenomenological theory of interpretation, the other a crypto-Marxist theory of social praxis and power. No matter how diverse and difficult to reconcile, these two philosophical positions shared with each other a decided emphasis on the situated place of the researcher in his or her own historical context—for Gadamer this formed no inhibition but the very condition for understanding, for Adorno and Horkheimer this meant that scientific practice always implied social praxis in the present. Shanks and Tilley argued that archaeological understanding of the past was inherently conditioned by present circumstances but that it could and should at the same time be accommodated to question the contemporary world.) The hermeneutic perspective was further elaborated in books like *Reading Material Culture* (Tilley 1990), *Material Culture and Text* (Tilley 1991), *Interpretative Archaeology* (Tilley 1993), and *Interpreting Archaeology* (Hodder et al. 1995). Tilley's work on Swedish rock art showed how a structural-Marxist approach to material culture could be combined with such a hermeneutic perspective (1991; cf. Kolen 1992).

Post-structuralism, largely inspired by the deconstructivist work of Derrida, was explored in an edited volume called *Archaeology after Structuralism* (Bapty and Yates 1990) and also in *Reading Material Culture* (Tilley 1990). Unlike structuralists, post-structuralist thought refused to find immutable structures underlying cultural expressions but focused on the endless play of semiotic referencing between signifiers. It advocated the absolute primacy of the text, the author being decentred, the semantic anchor being lost: the signified, the actual meaning of the sign, was bracketed in favour of the autonomy of the signifier.

Thomas (1991: 4) said that there were no longer ‘fixed points of reference [...] which might act as Ariadne’s thread to guide us through the labyrinth [...]. Now, there is only the labyrinth.’ Interpretation was, therefore, fluid, uncertain, and on-going. Although a consequent application of such deconstructivist paradigm led to a self-effacing of archaeology itself (Yates 1990), post-structuralism made a decided impact on archaeology. It brought about the idea that material culture, to be adequately understood, had not to be translated into a verbalized, codic, discursive meaning or ultimate signified but could be interpreted on the level of practical consciousness, non-discursive meaning, shifting signifiers and intrinsic material meaning (Hodder 1989). Hodder, in particular, was for a while seduced by a mild form of post-structuralism (not the radical deconstructivism of Derrida): his *The Domestication of Europe* (1990) was not just an interpretation of the Early Neolithic in Europe, but also a literary and stylistic exercise which explicitly problematized the author-text-reader triad, which decentred the subject of the author and questioned ‘whether there is anything of the ‘I’ in this book at all’ (x), and which emphasized ‘structural indeterminacy’ and ‘interpretive uncertainty’ (279, 310) without becoming ‘a decontextualized post-modern pastiche’ (279).

Thirdly, there was a political reason, closely related to the above theoretical reorientation, which influenced the decline of ethnoarchaeology. The incorporation of this amalgam from philosophy, social theory and literary criticism brought about a textual turn in archaeology: post-structuralism proclaimed the absolute autonomy of the text, divorced from its author and open to multiple readings (‘There is nothing outside the text,’ said Thomas (1991: 4); cf. Yates 1990); hermeneutics, on the other hand, used the reading of a (historical) text as the paradigmatic example of all other forms of interpretation. The text metaphor increasingly came to dominate archaeological discourse (Hodder 1989): material culture was no longer defined as a meaningfully constituted instrument of social negotiation but was now looked at in terms of a textual analogy, ‘as a discourse that is always already written, which the investigator reads and then subsequently rewrites and translates to produce his or her text’ (Tilley 1991: 180). Like a text, material culture was not just bestowed with intentions but always open to novel interpretation, both for past actors and for present scholars.

This contradicted earlier views on a number of essential points: Marxists and structuralists had still been convinced that an adequate and ultimate understanding of the past was possible, even if this was never easy. ‘However “other” it seemed at first,’ Hodder (1986: 127) had stated, ‘an evaluated approximation to understanding is feasible.’ Indeed, sufficient methodological rigour allowed to reach the hidden meanings (structuralism) and hidden agendas (Marxism) of life in the past. Archaeologists could thus situate themselves on a privileged, Archimedean vantage point from where the mechanisms of symbolic codification and social manipulation were intelligible. Post-structuralist and hermeneutic archaeologists, on the other hand, agreed that the interpreter was always implicated in the process of understanding, that he or she was inextricably bound up with the very act of making sense of a text. Interpretation as an ongoing process was, therefore, never fully accomplished, as every new signifier was related to other signifiers and as the

hermeneutic circle could never be closed. Whereas contextual archaeology withheld an aftertaste of the positivist optimism in its study of ideational and ideological aspects, post-processual research worked with lesser degrees of epistemological confidence.

It was in this context that post-modern issues of epistemic relativism, narrative fragmentation, plurality of interpretation, and multiple readings of the evidence were embraced—which led to a questioning of the legitimacy of the archaeological discourse as the sole authoritative voice on the past, a critical study of archaeological praxis in the present, and an interest in alternative claims on heritage resources. Apart from a consideration of non-professional views at sites like Stonehenge and feminist re-interpretations of the androcentric master narrative, this also entailed a greater attention to indigenous perspectives on the historical landscape in the post-colonial context. Land claims made by Australian aborigines, the demand for reburials by native Americans, ethnic protest in New Zealand against the display of skeletal material in museums, all such cases showed the political nature of archaeology. They re-shuffled the ‘us versus them’ dichotomy inherent in much ethnographic and ethnoarchaeological reasoning. Anxious to respect the variety and uniqueness of cultural identities, post-processual archaeology eschewed the search for cross-cultural regularities and other homogenizing tropes. Gosden (1999: 9) has recently said that ethnoarchaeology is ‘immoral’. The particular, the specific, the unique, the idiosyncratic, the contingent, this became the only theoretically acceptable and politically correct field of research. If contemporary non-industrialized societies were consulted, it was not so much to upholster archaeological constructs but to deconstruct the taken-for-granted dominance of Western academic thought. Ethnoarchaeology, with its time-honoured cross-cultural ambition, could no longer function as a source of direct information:

Ethnoarchaeology must be kept at a discreet distance from archaeology as itself, the inappropriateness of direct, cross-cultural, cross-temporal analogy (which of course means the destabilising effect of an (ethno)archaeology of here and now) requires the ethnographic evidence to be used for the building of general theory.
(Moran and Hides 1990: 215)

Ethnoarchaeology thus lost its specific *raison d'être* with the stress put on historical uniqueness and interpretative openness. Ethnographic evidence on material culture could only be subsumed in a wider framework of anthropological theory-building, not in the specifics of archaeological explanation. Thomas, inspired by Ricoeur, regretted that ‘the great bulk of archaeological writing [was] conducted under the sign of the Same’ (1991: 3) All too often, he argued, archaeologists made use of ‘some form of universalism, whether it is called analogy, uniformitarianism or middle-range theory’ (3). He advocated ‘an archaeology of difference’, ‘a contrastive history’, which entailed ‘the recovery of temporal difference’ and maintained ‘the strangeness of the past, its alien quality’ (5). By giving a false impression of familiarity, ethnoarchaeology only removed the past’s alterity.

Inspiration, speculation, reification

Rhetorically redundant, theoretically problematic, and politically unwanted, ethnoarchaeology suffered from the conceptual climate which stressed the culturally unique over cross-cultural generalization, difference over similarity, open interpretation over precise inference; a climate which also proclaimed a radically different perspective on non-western others. However, whereas many post-processualists like Thomas, Barrett and Hodder (after 1986) avoided an ethnographic impetus altogether, some still incorporated lengthy descriptions on material culture in contemporary non-industrialized societies in their archaeological texts—albeit for entirely different purposes. The later work of Chris Tilley, consisting of three monographs published in the 1990s, is a case in point. His study of prehistoric rock carvings in northern Sweden contained sections on the ritual cosmology of the Evenks from west Siberia and on the contemporary rock-art from aboriginal Australia (Tilley 1991: 126-37; 164-7). He wrote: ‘The use of ethnohistorical data is, of course, fraught with difficulties and can only take us further in an hermeneutic appropriation of the meaning of the carvings to a limited degree’ (1991: 136). In contrast to the contextual publications, his argument did not draw on extrapolations from personal ethnoarchaeological fieldwork but used ethnographic and ethnohistorical literature as heuristic devices in his endeavours at hermeneutic understanding. Tilley’s subsequent book on prehistoric landscapes in Britain contained a substantial chapter on Australian and Alaskan representations of landscape, highlighting its affective, emotional and symbolic significance (Tilley 1994: 35-67). Again, what Tilley attempted to do was not so much finding straightforward ethnoarchaeological parallels or principles of behaviour like material-culture strategies, but assembling, rather impressionistically and selectively, a number of ideas about landscape which were very different from a functionalist, industrialist point of view. Ethnography was an escape route to get out of one’s own eurocentrism—but by treating non-Western others as a unitary category it ran the risk of even reinforcing it. In his latest book on metaphors and material culture, Tilley included a chapter on contemporary Wala canoe building in Melanesia, based on ethnographic fieldwork (Tilley 1999: 102-32). Here, the study no longer fulfilled an ancillary role to archaeology, but stood on its own, side to side with other chapters on prehistory and material culture theory. Tilley’s work thus reveals how the ethnography of material culture could be invoked as a source of inspiration for archaeologists but also how it could become a research field in its own right.

Those post-processualists who still admitted an anthropological input in their interpretations, generally restricted it to inspirational purposes. The most extreme example of this comes from a recent article on Stonehenge published in *Antiquity* by the archaeologist Mike Parker Pearson and Ramilisonina, an inhabitant from Madagascar who ‘has lived his life in communities which regularly erect standing stones’ (Parker Pearson and Ramilisonina 1998: 309). Initially based on a BBC documentary, Ramilisonina’s visit to Europe’s premier megalithic monument led to a scholarly publication on the meaning of Stonehenge. The authors recognized four different but convergent analogies. There was a ‘formal analogy’ between

Neolithic Britain and contemporary Madagascar in that megalithic construction took place in both (which is more an observed similarity than an analogy). They further saw a ‘cross-cultural analogy’ in the social significance of ancestors, ‘a phenomenon found in many societies’ and therefore probably present with ‘the people of Late Neolithic Wessex’ (310; note that this is rather a cross-cultural, descriptive generalization than an analogy). They also stressed the ‘relational analogy’ in Madagascar between standing stones and ancestors: such stones are erected after death and become the places where contact with the deceased is sought (again, this is a description of one particular context rather than an analogy between two contexts). Finally, they presented an ‘analogy of materiality’ which stressed the inherent physical qualities of stone like durability, hardness, and imperishability as natural, universal metaphors for concepts like eternity and immortality (a metaphorical quality to guarantee uniformitarian bridging). It was actually the combination of these four lines of evidence which resulted into one analogy: given the standing stones in Stonehenge and Madagascar, given their ancestral meaning in Madagascar, given the global importance of ancestors and universal metaphorical properties of stone, the conclusion was clear—like Malagasy standing stones, Stonehenge was built for the ancestors. Compared to the analogous wooden architecture of the living from nearby Woodhenge, Stonehenge presented a ‘lithicized’ version for the dead (a similar duality was found in Avebury). It was not the place for processions, rituals and feasting, such as current opinion holds (in particular Barrett). On the contrary, the lack of large quantities of rubbish marked it as a locale strictly reserved for the ancestors.

Although the authors indicated major differences with Madagascar (where standing stones are fairly small and related to individual ancestors, as opposed to the massive constructions for collective ancestors in Neolithic Britain), and though they relied on strictly archaeological arguments as well (the wooden similes unearthed near Stonehenge and Avebury, the absence of rubbish), their entire argument still hinged in a Sollas-like fashion upon the Malagasy analogy of ancestral worship and stone erection. Because if ancestor worship is truly quasi-universal, and if stones naturally refer to eternity, then one is surprised that only certain societies erect stone structures for the dead. The whole argument rested on the unquestioned assumption that remembrance is associated with monumentality in durable materials, a point on which Western and Malagasy society happen to converge. However, it is forgotten that in many non-western societies, remembrance can also be achieved through destruction of material culture. It suffices to recall the case of the elaborate Malangan sculptures in Melanesia which are destroyed shortly after the funerary ritual to realize that ritual destruction can be as good a mnemonic device as monumentality (Rowlands 1993). What is dressed up as four independent and converging analogies in Parker Pearson’s and Ramilisonina’s argument rapidly boils down to one formal analogy: the observed similarity between source and target (stone erection in Neolithic Britain and contemporary Madagascar) leads to a predicted similarity (ancestral worship at Stonehenge) on the basis of such accidental linkage in the Malagasy source. The article shows what the excesses of post-processualism can lead to: an attitude to ethnography which

is at best selective and impressionistic, and at worst gratuitous and remarkably ignorant about the logic of analogy.

Even in Palaeolithic archaeology, the field least affected by the post-processual vogue, such haphazard use of analogy re-emerged, particularly in the interpretation of cave art which is traditionally one of the ‘softer’ realms of Palaeolithic research. In a heavily mediated monograph, French rock art specialist Jean Clottes and South-African anthropologist David Lewis-Williams (1996) argued that prehistoric rock art could best be understood as shamanic practice. The geometrical, abstract and fantastic depictions would in their view represent the different stages of shamanic trance; each stage of altered consciousness giving rise to particular visual experiences. Since shamanism was argued to be a ubiquitous phenomenon on a global scale and since the modern human mind had like workings everywhere, their dual neuropsychological and ethnological approach enabled to speak about shamanism for the Upper Palaeolithic. These universal warrants notwithstanding, in practice their argument came down to expanding Lewis-Williams’ research on shamanism in San rock art to Clottes’ intimate acquaintance with French cave art. So doing, they literally repeated Sollas’ juxtaposition of Bushmen and Aurignacian artistic expressions. Sure, the book had the usual warnings that one should not paste the present onto the past, but just as with Parker Pearson and Ramilisonina’s paper, their argument projected one specific ethnographic context onto a past reality, dressed up with universalist claims and devoid of any serious discussion on analogy. Regardless of the possible merit of their interpretation, one is surprised to read their succinct statement on ethnographic comparison which regrets the presence of exact parallels and argues that only ‘comparison of what is comparable’ (Clottes and Lewis-Williams 1996: 64) is legitimate: a blatant recourse to formal similarity—as if there had never been an ethnoarchaeological discussion on analogy at all. Paul Bahn (1997: 67) rightly criticized their ‘use of ethnographic analogy to achieve a “best fit hypothesis”’:

Far more caution and rigour are needed to avoid the abuses of ethnography seen earlier this century, as well as the simplistic wholesale transfer of specific interpretations from one body of evidence to the other in what has been called ‘ethnographic snap.’

Bahn found that much of the shamanic theory ‘sounds suspiciously like the simple projection of the view of 20th-century post-processualists into past’ (66).

Ethnography nowadays seems to serve an inspirational purpose comparable to the one it had in the 1960s. Even the cautionary note has re-emerged from its ashes now that the links between archaeology and anthropology are loosened! In his acute article ‘Questions not to ask of Malagasy carvings’ the anthropologist Maurice Bloch (1995), who, like Parker Pearson worked with art in Madagascar, outlined the difficulties encountered in interpreting wood decoration as ‘a cautionary tale [...] of the attempts at interpretation’ (1995: 212). Cautionary tale or creative inspiration, the function of anthropology in the archaeology of the 1990s strongly recalls the situation of the mid-1960s, in particular also because of its poor consideration of the issue of analogy. But where the early New Archaeologists

eventually sought to improve their inferences by testing, post-processualists were totally unconcerned with the mechanisms of verification. And this, in fact, shows how the work of the latter not only echoes the 1960s but also the 1860s. Its glossing together of contemporary non-industrialized perspectives with prehistoric contexts and its covert use of projective reasoning remind even more of the late nineteenth-century, socioevolutionist logic. Of course, post-processualists make every effort to repudiate the cross-cultural, racial and colonial discourse, but their selective use of ethnography results in a similar creation of a homogeneous, unitary category of the non-modern, non-western other which embraces both present and prehistoric people. ‘Alterity’ functions as an umbrella term for very different sorts of human experiences. Despite its explicit avoidance of essentialism and dichotomous reasoning, the inspirational function of ethnography precisely affirmed such thinking! In the absence of a broader conceptual framework, this browsing in the ethnographic world remained haphazard: more often than not it simply found what it was looking for. And it reified the old Western-non Western dichotomy in a way hardly seen since the Victorian evolutionists.

Modern material culture studies

On the other hand, the autonomous study of material culture has in recent years emancipated itself into a rich and autonomous field of social and cultural anthropology. Stimulated by contextual ethnoarchaeology, it is still appreciated and frequently conducted by archaeologists like Tilley in his latest book. The same holds true for modern material culture studies: originally a part of the ethnoarchaeological fabric (not only in the work of Hodder and Miller, but also in the writings of Schiffer, Rathje and even Ascher), it became a distinctive research field in its own right, particularly through the writings of Daniel Miller who called for ‘an independent discipline of material culture’ (1987: 112; cf. 1994). Whereas a ‘post-processual ethnoarchaeology’ was never really aimed at (despite the vain attempts by some North-American scholars, cf. David 1992; Stark 1993), the study of material culture in modern and non-modern societies burgeoned as never before in archaeology and anthropology. In 1996 a new journal was launched, the *Journal of Material Culture*, and even before the first issue appeared it counted more than 600 subscribers. It was the British, post-processual answer to the *Journal of Anthropological Archaeology*. Its editors were—not surprisingly—Daniel Miller and Christopher Tilley who wrote that the editorial policy aimed to bring together ‘interdisciplinary research [...] on the ways in which artifacts are implicated in the construction, maintenance and transformation of social identities’ (1996: 5). Besides a relatively small number of archaeological papers (often on aspects of later British prehistory) which are invariably devoid of ethnoarchaeological inputs, the first volumes presented articles on themes like ‘objects and subjects in windsurfing’, ‘Vietnam Zippos’, and even ‘the material culture of tampons and napkins’. By the end of the twentieth century ethnoarchaeology, once an isolated subfield of processual craftsmanship, had been transformed into material culture

studies, a discipline in which prehistoric archaeologists, social anthropologists, sociologists of modernity and contemporary historians energetically engaged (Van Dommelen 1999, *in press*).

An age of extremes

The period after 1982 was by all means an age of extremes in the appreciation of ethnographic analogy. From a North-American field of technical and polemical middle-range research predominantly on contemporary hunter-gatherers undertaken to elucidate the more mechanical formation processes of Palaeolithic archaeological records, over a British exercise in general theory building conducted amid modern societies of pastoralists and agriculturalists and applied in studies of recent European prehistory, to material culture studies, a new branch of social science research that became increasingly detached from archaeological implications, ethnoarchaeology took on many forms as it went through the hands of processualists, contextualists and post-processualists. These differences were framed by a shifting theoretical perspective from functionalism over structural-Marxism to post-structuralism and hermeneutics.

This is readily observable from the notion of causality. Processual ethnoarchaeology had been successful in isolating causality as the most important criterion of analogical reasoning: relevant linkages within the source context were first deemed necessary before an extrapolation towards the past could be made. The insistence on finding such unambiguous and ultimately uniformitarian linkages led to an increasingly restricted area of research where causation was mechanical and a-historical. Contextual archaeology drew attention to specific historical contexts, to the particular ways in which general symbolic structures were appropriated in social action. Causality between behavioural dynamics and archaeological statics became more complex, since it resulted as much from historical norms and values as from general processes, and since material culture was no longer simply the product but also the producer of social action. Post-processual archaeology, finally, eschewed the notion of causality altogether. Since interpretations took on the form of ongoing understanding and making sense of things which were never quite certain and unambiguous, not even to the historical actors themselves, explaining cultural practices in causal terms was no longer a desideratum. It is this changing appreciation of causality which determined the fate of ethnoarchaeology: the possibility of drawing inferences about past realities from observations in the living world had always required a certain confidence in the existence of relevant and recurrent correlations between distinct aspects of the source. Since contemporary archaeology was reluctant to admit this, studies reduced the function of anthropology to merely inspirational or critical purposes, even if this has resulted in the creation of an essentialist notion of the non-modern other.

However, if there were major divergences in what archaeologists with differing theoretical backgrounds did, there was also a structural convergence in what they did not do: discussing analogy. After the heightened logical awareness in the late 1970s and after the groundbreaking publications by Wylie in the early 1980s, the debate on the proper use of analogy ended abruptly. Processual eth-

noarchaeologists stopped discussing the fundamental principles of their work and fastened their teeth into a strenuous boost of puzzle-solving activity. Contextual ethnoarchaeologists pledged allegiance to the plea for relational analogies but in practice often turned to the very sort of cross-cultural generalizations they loudly purported to avoid. Most post-processual archaeologists considered analogy as a non-issue, even if they resorted to an impressionistic borrowing from anthropological literature.

Ironically, the history of debate described here ended with the absence of debate. Ethnoarchaeology was not, or only very rarely, explicitly criticized, it just disappeared out of sight. The argument by analogy was not overtly rejected, it was just no longer used. It recalls Max Planck's bitter statement that older ideas are not abandoned because they have been disproved, but because they have become obsolete and their advocates cannot or do not any longer participate in the debate. Contextual and post-processual archaeologists, while generally open to discuss whatever theoretical topic, were remarkably silent on ethnoarchaeology and ethnographic analogy. Describing their attitude towards these themes consisted of documenting the pattern of its gradual disappearance and linking it to the central notions of their theoretical perspective. As such, the stress on nonverbal and nondiscursive practices they defended as essential to an understanding of people in the past equally applies to their own history.

Conclusion

It has become customary to describe the history of twentieth-century Anglo-American archaeology by means of a chronological division into three subsequent stages of theoretical development going from culture-historical archaeology over processual archaeology to post-processual archaeology. Whereas the didactic qualities of such scheme are undisputed, the formulaic repetition of this tripartite chronology and its forced application to regional traditions outside the English-speaking world (cf. Hodder 1991; Slofstra 1994) risk to turn it into a historiographical variant of a dogmatic and reified three-age system (Van Reybrouck 1994b). In the preceding sections I have found it necessary to nuance this classification in a number of ways. First, by showing that there was often continuity when discontinuity was claimed: the early uses of ethnography by the New Archaeologists often continued the direct-historical method of the traditional Americanist archaeology they purported to reject; the implicit cross-cultural analogies the contextual archaeologists resorted to carried on the generalizing and nomothetic efforts of the processualists. Secondly, the opposite was also true, i.e. there was often discontinuity when continuity was claimed: culture-historical archaeology was a retrospective name for the research of the first half of the century, a label which lumped together and masked the very real discontinuities between Kossinna's *Kulturreislehre* on the one hand and Clark's ecological archaeology on the other; the New Archaeology of the 1960s was quite distinct from its processual sequel in the next decade, despite the alleged continuity between the two; similarly, early contextual archaeology was much more different from post-processual archaeol-

ogy than current partisans tend to acknowledge. Rather than a tripartite scheme for the development of archaeological debate, at least a six-fold division would be needed to do some justice to the intricacies of the disciplinary history in the Anglo-American world. This would entail a strict culture-historical archaeology (Kossinna, the early Childe) for the first quarter of the century; a tendency towards functionalism (Clark, the later Childe) and humanism (Wheeler, Hawkes) shortly before and after the Second World War; the New Archaeology of the 1960s and early 70s (the early Binford, Clarke, Fritz and Plog, Watson, LeBlanc and Redman); the processual movement of the mid and late 1970s which continues in certain fields up to this day (later Binford, Schiffer); the contextual archaeology of the early 1980s (the early Hodder, Moore); and the post-processualist school proper which emerged in the late 1980s (the later Hodder, Shanks, Tilley).

Whereas processualism is now stereotypically associated with ethnoarchaeology and the culture-historical and post-processual traditions with the absence of it, this refined framework allows a more detailed appreciation of the role of ethnography in the history of archaeology. Clearly, rabid culture-historians refrained from using the evidence from contemporary analogues, as typological classification and artefact seriation were considered the appropriate and sufficient avenues for detecting migration and diffusion. Functionalists like Childe and Clark, however, accorded a certain, if limited, importance to ethnography. No matter whether the perspective taken was Marxist or ecological, both renounced the sweeping cross-cultural comparisons of the nineteenth century and believed that in cases of historical continuity, or else when ecology, economy and technology was fundamentally similar, present sources could be invoked to clarify some basic questions of function and organization. Such direct-historical approach was also popular in mid-century North-American archaeology. Ascher's reaction and call for reassessing the more general-comparative approach was not immediately followed by the New Archaeologists who often stuck to the safer criterion of historical continuity. Independent testing and critical warning were two procedures they used to circumvent the problematic issue of cross-cultural analogy. It was only when the impossibility of such testing and the wearisome nature of such cautionary tales were exposed that ethnoarchaeology started to play an essential role in the elaboration of processual thought. The attempts at law-building, the importance of falsification, the development of middle-range theory and the emergence of taphonomy were all indebted to work in ethnoarchaeology. Yet the yearning for logical certainty restricted the scope of research to the realms of unambiguous and uniformitarian causation. In later years, therefore, the field became restricted to the actualistic study of mechanical site-formation processes. At the same time, contextual archaeologists, inspired by certain trends in social anthropology, used ethnoarchaeology in a looser, less rigid sense as it helped them to discover and distribute an alternative perspective on material culture. Once this approach developed into a post-processual school influenced by a philosophy and literary criticism which stressed alterity and particularism, ethnoarchaeology disappeared out of sight. The contrast with processual archaeology is remarkable: whereas processual archaeology had at its start no immediate need for ethnoar-

chaeological studies, they were the sine qua non for the development of early post-processual thought. The equation of ethnoarchaeology with the entire two decades of processualism (New Archaeology and processual archaeology proper) is, therefore, faulty; so is the equation between the lack of ethnoarchaeological attention and the whole of post-processualism (contextual archaeology and post-processual archaeology proper).

To summarize, ethnography played its most decisive roles in functionalist, high processual and early contextual archaeology, even if that role was a different one for each period. In order to appreciate these differences, as well as the mutual similarities, it is now time to list the six strength criteria of the argument by analogy, as we have done so far for primate models and evolutionist ethnographic parallels.

The strength of ethnoarchaeological analogies

Firstly, it is important to ask to what extent the analogical arguments were strengthened by increasing the *number of source contexts*. Whereas most nineteenth-century scholars had searched the ethnographic record for the best fitting instance, in the twentieth century there was a growing tendency to consider more sources at the same time without privileging the one over the other. Those stressing historical continuity as the basis for analogy were obviously still limited in their choice of source analogues. Clark, for instance, could only invoke contemporary rural Finland to illustrate prehistoric Denmark. Watson had to study modern Iran to clarify early villages in the Near East. Hill and Anderson could only rely on modern Pueblo culture in their interpretation of the prehistoric Midwest. The direct-historical approach still strove to find the one and only source analogue in the present. The archaeologist had to scan the ethnographic record for models and 'then select that one, or ones, to best fit in his particular situation', as Stanislawski recommended (1974: 20). This changed with the hunter-gatherer ethnoarchaeology of the processual heyday. Although for obvious practical reasons ethnoarchaeologists typically studied only one or two foraging societies (Yellen the !Kung, Gould the Ngatatjara, Binford the Nunamiut and to a lesser extent to Alyawara, O'Connell the Alyawara and the Hadza), the eventual aim was to juxtapose these individual findings in order to increase the number of source contexts. In fact, none of the processual ethnoarchaeologists proclaimed 'his' society as the most archetypical, the best representative or the closest approximation to life in the Palaeolithic. On the contrary, Yellen found it 'extremely acute' (1977: 5) that his sample size was limited to one source, as it run the risk of ethnocentricity. For Binford it was essential to confront his Nunamiut data with the !Kung data in order to find patterns of variability and causes of variation. (His entire forager versus collector dichotomy was based on such a comparison.) This eagerness to expand the source context was epitomized by Whitelaw's work which did not draw on one, two, or a couple of modern analogues but entailed a study of no less than 800 settlements from 112 contemporary or ethnohistorically documented foraging societies. Contextual ethnoarchaeologists, on the other hand, mitigated this numerical requirement and contented themselves with studying only a hand-

ful of ethnographic contexts. This restriction resulted not so much from a belief that a best source could be found, but from their wish to ‘move away from formal cross-cultural studies’ to a more careful consideration and understanding of ‘*why* material is patterned in a particular way in each cultural milieu’ (Hodder 1982a: 40, original italics). Moreover, since on a more abstract level such material-culture strategies were thought to be operative in all societies, past and present, industrialized or not, the study of one society allowed ‘to see the world in a grain of sand,’ as Blake would have said.

The *variety of source contexts* also augmented throughout the century. Advocates of the direct-historical approach were still preferring a limited sample of homogenous source analogues (like rural societies in the Celtic fringe), but later processualists enthusiastically welcomed heterogeneity. Variability was not avoided but actively sought. Whitelaw inflated his modern sample because ‘a wide cross-cultural dataset is essential’ (Whitelaw 1991: 141). Schiffer (1978) believed that both native and ‘Nacirema’ societies could be instructive to derive laws applicable to the Palaeolithic—such laws were believed to hold across the most diverse forms of society. Clarke even welcomed models from the physical sciences—molecules and magnets were as useful as hunters and gatherers. The contextualists, too, applauded diversity of analogues. Hodder believed that ‘Western society is as good a source for analogies as are less industrialised peoples’ (1982a: 40). Even if the quest for covering laws was no longer on the agenda, they nonetheless believed that there were general strategies of material culture appropriation. Hence the emphasis on ethnographic fieldwork in both modern and non-modern settings. The more varied the examples, the more compelling the concepts of material culture’s active role and meaningful constitution.

Admitting variation in the source context eventually decreased the attention for the criterion of *similarity*. Again, those reasoning from historical continuity still regarded similarity as the all-important yardstick. Clark and Childe first sought to establish as much common ground between two cultures before drawing analogies, if not in terms of direct descent then at least in terms of structural resemblance. The early New Archaeologists, too, stressed the amount of convergence between source and target. ‘Closeness of fit’ was still the criterion and the term used in Ascher’s new analogy (1961: 323), in Ucko’s search in the ethnographic literature (1969: 26), in Gould’s early ethnoarchaeology (1974: 37–8), and in Stanislawski’s pottery studies (1974: 20). Anderson preferred to reason from ‘similar or identical implements’ (1969: 134) and Stiles very explicitly stated that ‘the degree of similarity in the two sets of data would determine the probability of the analogy being correct’ (1977: 96). However, with the further development of processual thought, this reliance on similarity alone came under fire. Freeman (1968: 265) had already criticized ‘the use of assumed similarities’ in ethnoarchaeological reasoning but it was David Clarke who formulated the strongest opposition when he wrote that ‘goodness of fit is not sufficient justification for selecting one particular analogy’ (1972b: 40). In subsequent years, during the heyday of ethnoarchaeology, similarity was abandoned as the pivotal criterion for analogical reasoning. Gould made a 180-degree turn: ‘Ethnoarchaeology

should not be seen as an attempt directly to achieve a close “fit” between larger and larger numbers of resemblances occurring in both past and present-day cases. [...] Analogues, based as they are on resemblances, are self-fulfilling’ (Gould and Watson 1982: 374, 375). It was in this context that the number and variety of source analogues was increased and that the criterion of relevance was more fully discussed; formal similarity could simply no longer do. Binford wrote: ‘The simple faith that similar forms had similar causes seems to be a very naive position’ (1985: 581). Once the hallmark of ethnographic comparisons, similarity was now rejected as a reliable device. Contextual archaeologists did not set a high price on this criterion either. As their use of ethnography was on a more general level of theoretical development, proximity between source and target was not required. Only the most recent examples of ethnographic comparison strive again to find abundant resemblance—Clottes and Lewis Williams maintained that one could only compare what is comparable; Parker Pearson and Ramilisonina’s argument rested, after all, on ‘goodness of fit’. Showing some basic likenesses between San shamanism and prehistoric rock art and between Madagascar and Stonehenge is considered an appropriate and sufficient strategy of analogical reasoning, especially if these likenesses are dressed up as universal properties in terms of basic neuropsychological principles or natural inherent metaphorical qualities.

The ability to deal with *dissimilarity* was also largely dependent upon the eminence one attached to similarity. Authors who highly valued the amount of resemblance were of course at pains to avoid, minimize, or conceal the discrepancies between source and target. Authors, on the other hand, who did not search for a close fit, could happily accommodate and even appreciate differences. Gould’s argument by anomaly, despite its misleading name, consisted in fact of analogical reasoning which respected differences. He wrote that ‘the differences or contrasts arising from such comparisons could prove more rewarding than the similarities’; similarities ‘could only confirm what one already knew’, but contrasts ‘could force us to recognize how the prehistoric past may have differed from present-day analogues’ (Gould 1980: 35). Yellen, too, accepted the unlikelihood that present foragers would replicate the past, but continued doing ethnoarchaeology since divergence between source and target analogues was not believed to be detrimental to it (1977: 4). Murray and Walker knew that ‘the past was not necessarily at all like the present: things became extinct’; they therefore named ‘the implicit awareness of the existence of difference [...] most important’ (1988: 275, 262). Gifford-Gonzalez concurred with that view when she wrote that ‘analogues are not expected to be homologues, [...] the differences may in fact be enlightening’ (1991: 221). The whole point in zooarchaeological research, she continued, was to see whether these differences were threatening the analogy or not. Bones from a modern reference collection might differ markedly from fossil ones in weight, colour, and chemical composition: ‘However, we deem these traits irrelevant [...] because they reflect processes which affected the ancient bone postmortem’ (225). Differences were weighed in terms of relevance.

This brings us immediately to the criterion of *relevance*. To Gifford-Gonzalez ‘our security in these analogical inferences stems from a background knowledge [...] of the causal and functional links’ between the relevant variables in the source (1991: 225). Analogies were successful to the extent that ‘the links between source or context on the one hand, and relevant criteria of resemblance on the other, are thus systematic and causally based’ (Gifford-Gonzalez 1991: 224). The attention for the vertical relation of causality, and thus for the relevance of similarities and dissimilarities reached an apex in processual ethnoarchaeology. Yellen’s laboratory approach, Binford’s focus on statics and dynamics, Schiffer’s behavioural archaeology, Gifford-Gonzalez’s zooarchaeology, all these perspectives were based on the idea that present processes had to be adequately understood before past predictions could be made. There was no point in projecting contemporary sources onto the Palaeolithic if the internal functioning of these sources was not clarified. No matter how many similarities source and target shared, in the absence of a consideration of relevance, they were pretty useless. Processualists went into such great efforts at analysing causality in the present that in the long run it became its sole focus; extrapolation towards the past being increasingly shunned. However, it would be unfair to accredit only processual archaeology with an interest for causality and relevance. Clark’s and Childe’s interest in analogy also attached relevance to the similarities reasoned from. It was because ecology, economy, and technology were regarded as the functional (Clark) or infrastructural (Childe) determinants of society that they were staged as the relevant bases for comparison. Contextual archaeologists, on the other hand, showed very little attention for this criterion. Believing that causality was not an appropriate term to speak about unique cultural contexts, the notion of relevance soon disappeared out of sight. The interest in the vertical relation of causality thus diminished, with the most recent uses of ethnography (on Stonehenge and shamanism) even narrowly relying again on the horizontal relations of similarity.

The attention given to the sixth criterion, the *weight* of the analogical conclusion, varied considerably during the century. Originally, the conclusion arrived at did not outweigh the premises of the analogy. Crawford only used analogy to infer the general function of otherwise enigmatic finds. Clark and Childe, too, suggested that analogy could only be invoked for interpreting the most mundane aspects of prehistoric life. Before the advent of the New Archaeology, the role of ethnographic comparison was thus situated at the lowest rungs of Hawkes’ ladder: it generally dealt with technological matters (attribution of function, mode of manufacture), occasionally with economic themes (Clark’s interpretation of coastal farmers), and only very rarely with social topics (Clark’s interpretation of Star Carr as a community). Processual ethnoarchaeologists who worked on hunter-gatherers were more eager to address economic and social themes like, on the one hand, resource strategies, exploitation systems, foraging modes, and, on the other, site structure, population density, and length of occupation. However, despite these higher ambitions, the analogical inferences did not become outrageous, probably because processual archaeology was so antithetical to speculation. Only when causes were mechanistic could very precise inferences be drawn; in

case of lesser inferential confidence, the weight of the conclusion diminished accordingly. Contextual archaeologists, however, were mostly far less prudent: specific material culture strategies could be displaced from one context to another, mostly on the basis of only assumed similarities or alleged universals; think for instance of Hodder's claim that in lineage-based societies, women typically use domestic architecture as a means of social expression. This being said, in most cases the conclusions remained vague enough to respect the balance between premises and conclusion. Hodder did never claim that in all agricultural societies pottery decoration was an instrument of identity bargaining; only that material culture was a powerful instrument of social negotiation. Less rigid than processualism, contextualists and later post-processualists were enabled to climb the social, symbolical and ideational rungs of Hawkes' hierarchy. It is no coincidence that the latest examples of ethnographic analogy dealt with cosmological themes related to megalithic monuments and rock art. It is no coincidence either that these examples severely imbalance input and output. It is quite a step to reason from the formal similarity between standing stones in Madagascar and Stonehenge to ancestral worship. It equally requires quite a leap of imagination to reason from visual resemblances between San rock art and Palaeolithic cave art to shamanic rituals, initiation rites, and membranous walls that separate the given world from the underworld. Indeed, such imbalanced analogies rapidly turn into a 'barrage of unfounded speculation,' as Paul Bahn named it (1997: 65).

Surveying how archaeologists in the twentieth century dealt with strength criteria of analogical reasoning, two marked shifts can be noticed. Whereas defendants of direct-historical analogy still relied on the horizontal relation of similarity, processual archaeologists turned to the vertical relation causality. True to their name, processual archaeologists were more interested in processes than in trait lists. Since the study of hunter-gatherers was one of their prime areas of interest, the entire issue of continuity and similarity was out of the question. Working on the Mousterian, the African Lower Stone Age, or the late Palaeolithic reindeer hunters, processual archaeologists had to devise ingenious strategies to circumvent the problem of reasoning from similarity. No one wanted to commit the same fallacies as Sollas had done, so other avenues of research were sought for meaningful extrapolation from the present. The processual interest for causality evaporated with the rise of contextual and post-processual archaeologies and despite claims to the contrary, analogical reasoning returned to cross-cultural projections, impressionistic sampling and inspirational purposes.

Optimism, pessimism and the redundancy of analogy

Kossinna and Binford might not be the most evident archaeological partners, but they converged on one crucial point: their unmitigated optimism as to the potentials of archaeological research. Kossinna's identification of '*Töpfer mit Völkern*' was closely paralleled by young Binford's trust that 'the total extinct cultural system' was represented in the archaeological record. In both cases, there were high hopes that what was looked for (cultures for the one, systems for the other) could

be reasonably well recovered from the evidence. Archaeology's material and formal object were experienced as being not far apart from each other. Sound archaeological methods, it was believed, were enough to bridge both ends. Ethnographic analogy was not really needed, in fact, its influence might even have distorted the study of the past.

It must be said that for most part of the twentieth century such optimism was rather rare and the dependence on analogy therefore more frequent. Indeed, if there is one general tendency in twentieth-century archaeology, it is that the relation between material remains and prehistoric people has become more and more problematic. Kossinna's equation of pots with people was already questioned in the Interbellum when 'the Indian behind the artefact' was sought. Material culture was once believed to represent people, now it was believed to hide them. The second quarter of the century was marked by an incessant drive to go beyond the study of mere objects to reach a fuller understanding of societies—as well as by the frustration of reaching such ambitious ends. It was in this context that the old procedure of ethnographic analogy was dusted, though all connotations with the sweeping nineteenth-century comparative method were avoided. Had the culture-historical equation between pots and people been questioned by functionalist and humanist archaeologists, it was rejected by processualists. Not so much for the obvious reason that they disliked migrationism and diffusionism, but because the relationship between behaviour and remains, between dynamics and statics, between systemic context and archaeological context became the prime focus of attention. Ethnoarchaeology provided a laboratory setting where this relationship could be studied *in vivo* and it turned out to be more complex, multifaceted and problematic than ever expected. As a consequence, contextualists eschewed the faith that causal linkages could be found, especially since they added that material culture also constituted the social order. Ethnoarchaeology had helped to develop such ideas, but was eventually dismissed in the particularist climate of opinion.

Ethnoarchaeology, and more broadly ethnographic analogy, thus thrived between two extremes: between the solid linkage of archaeological remains and prehistoric reality at the beginning of the twentieth century and the profound scepticism regarding the existence of such linkage at the end of it; between a vertical relation of causality that was depicted as bold line and one that was depicted without lines at all. Ethnoarchaeology required a dotted line, a minimum of trust in causal links that could be explored, analysed and unravelled. And this is where processual archaeology prospered—in the study of causality, in the assessment of relevant similarities and differences, in the increase and variety of source contexts, in the balance between premise and conclusion.

Primate models

The idea of a primate model

Why did it take several centuries to find out the possibility of polyphonic chant in the late ninth century? Why did it take so long before perspective drawing was discovered by Italian artists in the fifteenth century? Why was the bicycle only invented in the early nineteenth century? Looking at the history of Western art and technology, we are struck by the number of late appearances of what we now see as relatively simple innovations. The combination of several melodic voices into one song, the conventional representation of a three-dimensional world on canvas and the conjoining of two wheels with a simple frame all seem such simple attainments that, as soon as they exist, we start to wonder why it took so long to discover them and how we managed to live without for so long. However, we tend to forget that these inventions only rarely respond to a long-standing need; very often such a need existed only shortly before or was even created together with the invention. In fact, nobody longed to cycle before the nineteenth century: it took more than fifty years after the first bicycle was assembled that people commonly ventured on the wobbly vehicle. The invention of the bicycle was not the consequence of a public demand but its cause.¹

Much the same holds true for the idea of primate modelling, i.e. the insight that living apes and monkeys can shed light on the remote human past. It is hard to believe that such apparently simple idea was only very recently elaborated, in particular since popular culture has made us familiar to see chimpanzees as some living fossil ancestors. Yet the idea of a primate model, even when not restricted to chimpanzees alone, is of a surprising young date. Indeed, it was only in the 1950s that the relevance of nonhuman primates for understanding human behavioural evolution started to be systematically explored. In this introductory section, I want to briefly outline the historical pedigree of this idea and also indicate the contexts within which it was eventually ‘discovered’. I will argue that the need for a primate model, was only seriously felt after World War II, particularly in the

1 More than a didactic opener, my brief reference to the history and sociology of technology stems from the conviction that these studies are quite illuminating in understanding the history of science. ‘Technology is the shibboleth that tests the quality of science studies,’ Callon and Latour once wrote (1992: 358).

context of an ideological reformulation of the definitions of humanity and animality and the discoveries of fossils older than anything hitherto known.²

Nonetheless, this recent idea built on previous traditions of academic scholarship in disciplines such as anatomy, zoology and psychology, just like Renaissance painting was indebted to the canons of medieval and classical art or just like early polyphony drew upon traditional Gregorian chant. Though recent, the idea of primate modelling was not entirely new, but an innovative elaboration which incorporated older ideas in new contexts. Somewhat simplified, we can divide the pedigree of this idea into four episodes of unequal length, each dominated by a central assumption:

1. Antiquity to Enlightenment: the anatomy of primates can elucidate the anatomy of modern humans.
2. Nineteenth century: the anatomy of nonhuman primates can elucidate the anatomy of *fossil* humans.
3. Early twentieth century: the *behaviour* of nonhuman primates can elucidate the behaviour of modern humans.
4. Postwar period: the behaviour of nonhuman primates can elucidate the behaviour of *fossil* humans.

Such a step-by-step division entails the risk of suggesting a gradualist and presentist history of science which sees past knowledge only as a ladder to the present pinnacle of understanding. However, these episodes do not imply any ascending order of perfection; as periods characterized by one basic assumption, they rather resemble autonomous, Foucaultian *epistemes*. In fact, this periodization is closely related to the disciplines in which primates were successively studied, medicine, zoological systematics and natural history, psychology and anthropology. At certain moments of their development, these disciplines realized the value of studying primates. The rise of a new step does, therefore, not necessarily imply the end of the former; only that new disciplines became interested, building on previous scholarship on primates: primate anatomy was still studied in palaeontology after the nineteenth century, but psychology came in; psychologists were still interested

2 For this section I have mostly relied on the excellent, if often old, literature available. The classical study is by Yerkes and Yerkes (1929) who assembled all historical sources on anthropoid apes before the twentieth century. Relying on this work, Morris and Morris (1966) provide a useful overview, as do Ducros and Ducros (1995). On specific periods, Jennison (1937) and McDermott (1938) focus on the ape in Antiquity and should be read along with Janson's superb study (1952) of simian representations in medieval and Renaissance art. For popular and scientific perceptions of primates after 1600 to the present day, much valuable information can be found in a volume edited by Corbey and Theunissen (1995; see also Corbey 1996). The history of American physical anthropology, including primatology, is well known through a special issue of the *American Journal of Physical Anthropology* (1981, 4) and through a volume edited by Frank Spencer (1982). Donna Haraway's baroque, controversial and sweeping *Primate Visions* (1989) is an incredibly rich but irritably dense authoritative source on primatology and ape lore in the 20th century. Michael Schwibbe from the Deutsches Primatenzentrum Göttingen very generously sent me his unpublished manuscript *Große Affe: Mythen, Fakten und Fiktionen* which presents a detailed history of Western understanding of great apes, a timely update of Yerkes and Yerkes (1929).

in primate cognition after the Second World War, but anthropology joined. This division suggests the rise of new ideas out of older ones, not the end of old ones as such.

First episode: from primate anatomy to human anatomy

Monkeys and apes figure in myths throughout the world. Their directly observable, uncanny similarities with humans, combined with their equally obvious differences, make them ideal creatures for mythologization. They provide a rich and complex texture on which multiple ideas about human nature can be projected. Whereas monkeys were worshipped in the ancient civilizations of Egypt, India and possibly Mesopotamia, in the Judaeo-Christian and Graeco-Roman tradition they were associated with trickery, mischief and lasciviousness (Jennison 1937; McDermott 1938; Morris and Morris 1966). Such extremely divergent cultural attitudes are typical for liminal beings which violate the boundary between man and beast, between civilized and savage, between culture and nature (Corbey 1993). The difficulty to assign them a place in our mental categories, i.e. their categorial ambiguity is eliminated either by worship or by scorn. Whatever attitude is taken, the mythological position of the primate is always defined in terms of its similarity to humans.

Early scientists were also fascinated by the likenesses between human and non-human primates (Spencer 1995). In his *Historia Animalium* Aristotle (384-322 B.C.) described the three known monkeys of his age—the cercopithecine, the baboon and the tailless macaque—noting how the interior and exterior of the animals resembled human anatomy. Galen (c. 130-200), the great physician from Pergamum, pursued this observation with an important practical consequence: when studying the anatomy of human corpses was prohibited in his time, he urged to dissect monkeys instead. His views on the human body gained so much authority that they would last unchanged for nearly 1500 years. It was only when Andreas Vesalius published his monumental *De fabrica corpori humanis* (1543), largely the result of his nocturnal escapades to the gallows and clandestine dissections of criminals' corpses, that Galen's conclusions proved to be wrong in several respects (Dougherty 1995). Galen's work, though, is important because it clearly shows how the anatomy of primates was thought to be relevant to understanding human anatomy. The study of monkeys was thus firmly rooted in the classical and medieval tradition of medicine. Throughout the Middle Ages, this anatomical similarity was acknowledged but the mental difference was thought to be fundamental. Most medieval theologians and artists regarded the monkey as a diabolical creature, a *naturae degenerantis homo*, a fallen man characterized by ugliness, untrustworthiness, lust and sin (Janson 1952: 73-94; 107-44).

With Vesalius' dissections of humans, it seems as if nonhuman primates were no longer needed to understand human biology. This would have been true, were it not that at the same time that Vesalius run his scalpel through human skin, exploratory travellers had come into contact with creatures which were even more humanlike than the barbary ape or hamadryas baboon thus far known in the West. The discovery of the great apes in early modern times posed a new challenge to

definitions of humanity and animality in Western thought (Janson 1952: 332-5). Portuguese Jesuits working in Sierra Leone during the sixteenth and seventeenth century wrote about the behaviour of chimpanzees, even including tool-use (Sept and Brooks 1994). In 1641, probably the first anthropoid to reach Europe was a chimpanzee described by the Dutch anatomist Nicolaas Tulp as an 'orang-outang'. And the Englishman Edward Tyson was the first to dissect a chimpanzee in 1699 (Montagu 1943; Nash 1995). He held that the animal was an intermediate (but not an evolutionary) link between man and ape, and added that all mythological creatures such as pygmies, cynocephali, satyrs and sphinxes were in fact monkeys or apes. From an instrument in the medical study of the human body and theological inquiry, in postmedieval times primate anatomy became an object of zoological attention and philosophical discourse about the limits of humanity.

Confronted with an increasing number of anthropoid evidence from chimpanzees, orangs, and also gibbons, scholars in the second half of the eighteenth century struggled over the taxonomic position of the great apes (Yerkes and Yerkes 1929: 16-26; Röhrer-Ertl 1988; Dougherty 1995). Gibbons, which had just been discovered, were not easily classified because of the great differences between the species. Chimps and orangs were often mixed up, supporting the long-standing theory on there being two varieties of orang-utan, the black in Africa and the red in Asia. The great sexual dimorphism in orang-utans and the fact that the skeletons of young orangs look more like chimp skeletons than like mature orangs obscured matters even further, especially since the skins and skeletons, that were imported drowned in alcohol or totally desiccated, were often of poor quality with vague indications of their actual place of origin. (With the Portuguese dominating the west coast of Africa, many imports from South-East Asia had to pass through their ports, so that 'Angola' was often given as place of origin of a specimen, even if this was not the case. Cf. Röhrer-Ertl 1988.)³ This confusion was only settled well into the nineteenth century, particularly through the anatomical studies of Richard Owen. The greatest taxonomic dispute in the age of Enlightenment, however, concerned the position of man *vis-à-vis* the great apes. In a very schematic way, one could say that scientists either lumped humans and anthropoid apes on the basis of observed similarities or splitted them apart on the basis of equally obvious differences. Among the first, Lord Monboddo included the orang-utan within the human species (Barnard 1995) and in his first edition of the *Systema Naturae*, published in Leiden in 1735, Linnaeus placed man and ape in the same order of Antropomorpha, later renamed as Primates. Linnaeus' contemporary, the great French naturalist Buffon, argued strongly against this lumping together because of man's unique capacity for reason. The presence of speech and hands led the German Blumenbach to devise even a separate order strictly reserved to humans. The debate was a particularly thorny one considering the wide-spread belief in eighteenth-century biology of what Lovejoy has called 'the Great Chain of Being' (Lovejoy 1936: 227-41; Günther 1907: 28-38): the

3 To complicate matters even further, Angola may have been also confounded with Angkola, a region in Sumatra where orangs are reported to have lived (Corbey, pers. comm.).

idea that all organisms in nature could be arranged in an infinite and continuous sequence, from the most simple to the most complex, a *scala naturae* devoid of any hiatuses. Much more than now, differences between great apes and humans were thought to be minimal—which explains the tug-of-war concerning the definition of what was human versus what was animal. The demarcation line between the Hottentots and the orang-utan (literally, ‘the man of the wild woods’) was experienced as rather thin.

Despite profound differences, the European reaction towards monkeys and apes before 1800 consisted mainly of coming to terms with their anatomical similarity to humans. Most authors acknowledged that the bodies of these animals were very close to their own physical constitution, but this idea was more than often minimized by stressing the mental inferiority of the nonhuman primates. Humans had ‘soul’ or ‘reason’ (the exact term depending on whether you were a medieval scholar or an eighteenth-century encyclopedist); apes and monkeys lacked such mental faculty. They were morally inferior, deprived of articulate speech and incapable of rational thought—ideas which echoed the pithecopobia of the Graeco-Roman and Judaeo-Christian tradition. It was this ambiguity of physical closeness and psychic distance against the background assumption of a *scala naturae* that led to the vehement taxonomical discussions in the second half of the eighteenth century.

With most evidence coming from dead rather than living animals, it is no surprise that research on nonhuman primates was by and large anatomical; behaviour was simply not yet at stake. Apes entered Europe as carcasses shipped in from Africa and Asia or, if alive, as curiosa displayed in menageries to be dissected after death (Zuckerman 1980). Frédéric Cuvier, the younger brother of the famous zoologist, observed the antics of a young captive orang-utan that was brought to Paris in 1810 (Martinez-Contreras 1996). Apart from the anecdotal and often fanciful accounts by travellers and zoo-keepers, very little was known about anthropoid behaviour. This led the eighteenth-century naturalist Hoppius, a student of Linnaeus, to say that ‘it would lead not a little to Philosophy, if one were to spend a day [with apes] exploring how far human wit exceeds theirs, what distance lies between Brutish and rational discrimination’ (quoted in Yerkes and Yerkes 1929: 19). Some French philosophers went even further by assuming that an understanding of the way primates lived might reveal something about man in his natural state (Hastings 1936: 109–32). Virey said: ‘Ainsi, l'*histoire naturelle des singes*, jetant une vive lumière sur celle de l’homme originel, est trop utile pour qu’on puisse la négliger.⁴ And in a note to his famous *Discours sur l’origine et les fondements de l’inégalité parmi les hommes* (1755), Rousseau even argued that some of the anthropoids such as the orang-utan were no apes but ‘*de véritables hommes sauvages*’, the original men who had not devolved into modern man but who lived ‘*encore dans l’état primitif de la nature*’ (quoted in Hastings 1936: 120). According to Virey and Rousseau, the lifestyle of primates had something to say about the

⁴ Wiktor Stoczkowski drew my attention to this line in Virey’s *Histoire naturelle du genre humain* (1800: 93–4).

human past. Yet it would be wrong to see them as direct precursors of modern primate modelling; such an evolutionist reading does not do justice to the intricacies of their thought and time (Moran 1993), nor to the contexts in which primate models emerged shortly after the Second World War.⁵

Second episode: from living to fossil anatomy

With Linnaeus placing man and ape in one and the same order, a first episode in the rise of the idea of primate modelling had come to a close. A second episode can be seen to have matured between the end of the eighteenth century and the second half of the nineteenth century when some authors developed the idea that the anatomy of modern primates was not just similar to *modern* humans but also instructive on *ancient* humans—who might have been very different from what we are now. Apart from medical and anatomical studies on primates, new fields like comparative zoology and palaeontology (disciplines often practised by scholars with a medical background) became interested in them. This interest was flanked by the rise of transformationist theories. Scholars like Aristotle, Galen, Tyson and Linnaeus had interpreted the anatomical resemblances in essentialist terms: to them, species were unalterable, natural kinds. And even if they ranked animals along a scale of beings (Lovejoy 1936), this categorization was strictly gradational and never evolutionary in that one species could not evolve into another (Spencer 1995).

The emergence of transformationist theories in the late eighteenth and early nineteenth century, such as Lacépède's and Lamarck's, allowed to see the anatomical resemblances and divergences as signs of changes through time. This vision led to Charles Darwin's controversial theory of evolution through natural selection. Although he hardly spent a single sentence in the *Origin of Species* (1859) on the issue of human evolution, to his supporters like Huxley and Haeckel it became clear that primate species were connected through ancestral links. Darwin himself turned to the subject in his *Descent of Man*, published in 1871. On the basis of embryology and comparative anatomy (much less on the basis of the scant fossil evidence), he concluded that 'man is descended from a hairy quadruped, furnished with a tail and pointed ears, probably arboreal in its habits, and an inhabitant of the Old World' (1871, 2: 389). Darwin provided his readership with a reconstruction of the anatomical peculiarities of 'our early progenitor', inferred from what he saw among embryos and extant primates. The emphasis was on anatomy; behavioural aspects only making an entrance in his study of facial expressions (1872). Drawing from his observations and minor experiments with the primates in the London Zoological Gardens, he suggested that humans' emotional displays paralleled the simian pattern and were therefore the result of evolution (Loy 1997). This was a rare case of evolutionary reconstruction in the nineteenth century based on behavioural observation, though 'behaviour' was here limited to

⁵ Rousseau's view of the natural state, for instance, is essentialist and does not have the temporal implication of human evolution.

such physiological aspects like the expression of emotion through body coloration and ear movements; it was definitely not *social* behaviour.

Darwin's impact was of course tremendous. Now, chimpanzees no longer simply looked similar to humans; they had actually been descended from the same progenitor. This made them promising anatomical models when the first human fossils were discovered. A good example of this use of chimpanzee morphology in a palaeontological argument comes from Fraipont's study of the Neanderthal skeletons from Spy (discovered in 1886): Fraipont investigated the Neanderthal posture by comparing the tibia from the Spy 2 specimen with a chimpanzee and human shin bone. He found the chimpanzee tibia most similar and concluded that the Neanderthal must have had habitually bent knees (Fraipont 1888). Although this conclusion was eventually refuted, Fraipont's method is important because it uses the anatomy of a modern anthropoid to make inferences about a fossil hominid, a technique which would soon become standard in human palaeontology. With the growing acceptance of evolutionary thought during the second half of the nineteenth century, similarities and differences between man and ape turned from structural to chronological, as if the dividing lines of the essentialist tradition had been bundled into one long time-axis.

It comes as a surprise that in Victorian interpretations of archaeological evidence there is so little reference to primate behaviour. In a time obsessed with finding a life image of our brutal and savage ancestors one would expect that orang-utans and gorillas (the latter were only first described in 1847 and brought to Europe in 1855; cf. Yerkes and Yerkes 1929: 31-5) would be welcomed as living precursors. In fact, they were—but only so in extra-scientific circles where novels, newspapers, narrative writing had their say. Edgar Allan Poe's *Murders in the Rue Morgue*, published in 1841, staged a gruesome and bloodthirsty orang-utan. Paul du Chaillu's vivid descriptions of his '*chasse au gorille*' (1863; cf. Vaucaire 1931) sent a shiver through his civilized readership in Europe and America. And *Harper's Weekly*, a popular illustrated magazine at the time, described the Neanderthal as a 'ferocious-looking, gorilla-like human being' (quoted in Trinkaus and Shipman 1993: 109). But when we turn to the scientists themselves, no behavioural parallels were drawn between primates and primeval humans. All great social evolutionists (Chapter 2) opted to confine their studies on the origin of civilization to what they saw as the lower limit of humanity instead of the upper reaches of the animal kingdom—a point forcefully made by the eminent historian of anthropology Stocking (1987: 176-7). Tylor's inquiry ground to a halt with the description of the Tasmanians as the lowest savages available. Whereas the eighteenth-century *savant* looked at what lay below the Hottentots (and found the orang-utan), the nineteenth-century scholar stopped at the lowest point of the human realm. How was this possible?

One reason might be that there were no reliable observations coming from decent fieldwork. This was true, but such fieldwork did not exist for ethnographic information either which laid at the basis of the parallels we encountered in Chapter 2. This being said, the quality of both types of observation was not entirely equal: ethnographic documentation was much more elaborate as it was gathered by observers who had often

extensive contacts with native populations; knowledge on primates remained confined to anecdotal accounts of short-term observations or second-hand information obtained from missionaries or natives. ‘Sound knowledge respecting the habits and mode of life of the man-like Apes,’ Thomas Huxley (1863: 24) regretted, ‘has been even more difficult of attainment than correct information regarding their structure.’ In order to explain the archaeological evidence and the development of human civilization, contemporary savages were apparently ‘savage enough’ to the eyes of late-nineteenth century scholars. Lubbock found it ‘very improbable that man can have existed in a lower condition than that thus indicated’ by contemporary races (1865: 475). Since the overwhelming evidence of prehistoric man consisted of stone tools, the link with savages and their material culture was much more easily drawn than with vague primates. Savages were generally believed to approximate nonhuman primates anyway, both in their anatomy and in their antics. So when given the chance between either multiple and detailed accounts of savage practices relevant to Palaeolithic remains or scarce and often fanciful descriptions of great ape behaviour, many authors chose the first option, especially since they saw themselves as real scientists working with reliable evidence, not stories.⁶ Huxley’s (1863: 54) judgement on Du Chaillu’s imaginative description of the gorilla was: ‘It may be truth, but it is not evidence.’ If an analogy explains the unknown in terms of the known, the nonhuman primates seem to have been too relatively unknown in comparison with non-European savages.⁷ This was to change slowly.

Third episode: from primate behaviour to human behaviour

At the turn of the century the first naturalistic field studies on primates were being undertaken in Africa by Richard Garner and Eugène Marais. Enclosing himself in an iron cage in order to watch chimpanzees in West-Africa during the 1890s, Garner was the first to study primate behaviour in the wild, although the chimps were quite reluctant to show themselves (Candland 1993). Right after the end of the Boer War (1899–1902), the South-African poet, journalist, lawyer, and morphine-addict Eugène Marais lived for three years with the chacma baboons in his native land. His publications, written in Afrikaans, received little attention from scientists; his most important manuscript was lost for more than forty years and

6 Huxley (1863: 24–54) drew together the available evidence. It was reasonable for the gibbon and the orang, but minimal for the gorilla and the chimpanzee. He based himself on the reports of Dr. Savage, a missionary who in the first half of the nineteenth century had collected information from the natives along the river Gabon. Huxley regarded him as an ‘excellent observer’ (46), but stressed that our knowledge of these apes was still ‘much in need of support and enlargement by additional testimony from instructed European eye-witness’ (25). Huxley realized, however, that only ‘once in a generation, a Wallace may be found physically, mentally, and morally qualified to wander unscathed through the tropical wilds of America and Asia’ (25).

7 Tellingly, if Victorian scholars had an interest in animal behaviour, it mostly concerned well observable animals like bees, ants and beavers. Morgan himself studied beavers, while Lubbock observed insects (Ingold 1988; Swetlitz 1988; Clark 1998). Huxley (1863) assembled the scarce reports on great ape behaviour in the wild (cf. previous note); Lubbock (1870) discussed with Argyll the tool-using ability of monkeys; Darwin (1872) observed primate facial expressions in the London Zoo; and Westermarck (1891) mentioned the parenthood among the great apes. Although all this material was believed to be promising, its importance in the reconstruction of prehistoric behaviour still was minimal compared to the ethnographic input.

when it eventually appeared in English, primatology had come of age independently (see Ardrey 1961 and his introduction in Marais 1969; but also Zuckerman 1976). Despite the originality of their work, Garner and Marais are now more reputed as pre-scientific eccentrics than as primatology's pioneers.

In the decades preceding World War II apes and monkeys were for the first time systematically studied, both in captivity and in the field (Maple 1979; Gilmore 1981; Ribnick 1982). Research, mostly conducted by comparative psychologists, was initiated in Russia, Germany and Britain; it was 'the era of maze learning, obstruction boxes, the delayed response, multiple-choice methods, and the temporal maze' (Lockard 1971: 19). However, it was the American Robert Yerkes who became the self-crowned and widely acknowledged founder of primate research.⁸ Yerkes was influential on two levels: he undertook experimental studies with captive chimpanzees and he stimulated naturalistic observations of wild animals. To perform his experiments, he constructed research laboratories in Connecticut and Florida where the 'psychobiology' of captive primates could be tested (cf. Bourne 1971: 72-149). (Psychobiology was the ill-defined term Yerkes used to designate his experimental studies of cognition, inventiveness and other stimulus-induced responses of captive great apes with the aim of finding the biological roots of human psychology.) To improve the breeding programme of his captive colonies Yerkes knew that more naturalistic information was needed. Although he never did fieldwork himself, he sent three of his students to the tropics: Nissen (1931) and Bingham (1932) studied the elusive chimpanzees and gorillas, while Carpenter (1934) observed howler monkeys in Panama. The latter's work turned out to be the most successful naturalistic primate study before the war.

Studying the behaviour and cognition of wild and captive primates was for Yerkes not an end in itself. Throughout his career he stressed how understanding the apes contributed to 'the problem of improving human nature and the possibilities of its solution' for the 'generally recognized human shortcomings, such as extreme selfishness, dishonesty, slothfulness, cruelty' (Yerkes 1943: 10). Since chimpanzees were 'psychobiological gold mines' (Yerkes 1943: 7), knowledge of their behaviour had an 'immediate serviceability' to problems of human welfare (Yerkes and Yerkes 1929: 590). Like Galen, Yerkes used primates to clarify aspects of modern humans, this time their psychology. Yet in his three classic works (Yerkes 1916; Yerkes and Yerkes 1929; Yerkes 1943), he never used his chimps to speculate about human evolution.

8 This is clear from the number of biographical studies (Carmichael 1969; Rohles 1969; Bourne 1971; and several articles in Bourne 1977; Nadler 1996) and his prominent place in introductory chapters of nearly all textbooks in primatology (cf. Haraway 1989: 59-83). When the Premacks developed an artificial language for chimpanzee experiments, they baptized it 'Yerkish'. His great impact on (American) primatology, however, overshadows the importance of other, less fortunate figures, such as the German Wolfgang Köhler who was forced to abandon his groundbreaking laboratory studies on the Canary Islands after the first World War and the Russian Nadie Kohts, whose work suffered from an obvious language barrier. Yerkes knew and admired their work—he was in close touch with Köhler and even published (together with a Russian colleague) an article on Kohts—but they were somewhat forgotten in postwar primatology, although in psychology the work of Köhler (1925) is acknowledged to this day (Glaser 1996).

The British scientist Solly Zuckerman was in many ways Yerkes' opposite. He was a zoologist (not a psychologist), interested in 'mammalian sociology' (not primate 'psychobiology'), who preferred to study the group life (not the individual cognition) of baboons (not chimps) in zoos and in the wild (not in labs). His most important book was entitled *The social life of monkeys and apes* (1932)—note the difference with Yerkes' *The mental life of monkeys and apes* (1916). As to the relevance of studying primates, he did not share Yerkes' optimistic hygienics of the human soul. He refused to see how simplistic parallels between primates and people could solve the political problems of our age. Because of man's elaborate social institutions he found it ridiculous 'to describe the social behaviour of even the most primitive savage in the same simple terms that are found adequate to describe that of the monkey' (1932: 18). He considered it equally worrisome that inferences about the past from living primates were often mere backward projections of the behaviour of one living species. To him, the only valid approach was to look for similarities among *all* primate species, not just the gorilla or the chimpanzee:

Thus only the behaviour common to all apes and monkeys can be regarded as representing a social level through which man once passed in the pre-human stages of his development. In the life of the monkey one may see a crude picture of the social level from which our earliest human ancestors emerged. But only that. The behaviour of the sub-human primate represents a pre-human social level, a level which, though without culture itself, seems to have contained the seeds that grew into the culture of primitive man. (Zuckerman 1932: 26)

Zuckerman's comment might seem a restriction, but in fact it was one of the first theoretical statements on the palaeoanthropological use of primate parallels. With much caution, baboons in the London Zoo and captive chimps on Tenerife could tell us something about the fossils from Peking and Piltdown.

Zuckerman wrote his critique in response to a paper by A.L. Kroeber, who was the very first anthropologist to look at great apes.⁹ In his paper 'Sub-human culture beginnings' (1928) Kroeber, who was quite impressed by the chimpanzee experiments of Köhler and Yerkes, sought to find the 'organic basis and origin of culture' (326). He made an inventory of what was known about chimpanzee psychology, tool use, lack of speech and even religious anticipations but concluded that chimpanzees and other great apes had thus far no culture or 'any such institutional residuum of unmitigatedly cultural material' (331). He noted, however, that chimpanzees were largely inventive (through play, competition, accident, synthesis, 'destructive impulse' or any other factor), but that their lack of speech inhibited the transmission and preservation of newly acquired traits, which was another prerequisite for true culture. Zuckerman (1932: 26)

9 Before Kroeber, anthropologists had generally not shown any serious interest in primate behaviour to the extent that the zoologist G.S. Miller (1928) found it necessary to summarize existing knowledge on apes as a corrective on anthropological speculations on the origin of human society. The tide, however, was changing: in 1930, even a 'culturalist' anthropologist like Franz Boas could declare that 'a considerable field of social phenomena does not by any means belong to man alone but is shared by the animal world' (1930: 201-2).

warned Kroeber against ‘the paucity of established facts and the frequency of anthropomorphic misinterpretation’—which was a bit superfluous since Kroeber had not drawn any specific conclusions about the past on the basis of chimpanzees. Instead he believed that further study of the anthropoids would be ‘invaluable in the illumination of the basic problems of anthropology and all the social sciences’ (1928: 341) and concluded nearly prophetically with the words: ‘We have only begun.’

The intimations of Kroeber and Zuckerman bridge the gap between our third and fourth episode. Before World War II, there was a proliferation of behavioural studies on nonhuman primates, which were generally motivated by the promise of gaining more insight into the human mental condition (Loy and Peters 1991). Although these studies took place within psychology, some anthropological interest in primates and how they might illuminate human origins emerged. However, as young scientists were summoned for military work during the war, the growing interest in primate behaviour was interrupted in the West until the mid-fifties. In the meantime, research on primates started quite independently in Japan (particularly on the indigenous Japanese macaque), but language difficulties prevented for many years a cross-fertilization of ideas with Western scholars (Frisch 1959; Kitahara-Frisch 1991; Asquith 1991; 1995; Fedigan and Asquith 1991).

Fourth episode: from primate behaviour to early human behaviour

Although the value of primates in the reconstruction of early human behaviour was already alluded to by Virey, Rousseau, Huxley, Zuckerman and Kroeber, it was only after the war that primate modelling gained full momentum. The emphasis shifted from psychology to physical anthropology, from laboratory to savannah, from work *on* primates to work *with* primates (Schultz 1971: 7). Modern means of transport such as the aeroplane and the jeep together with advances in tropical medicine considerably facilitated fieldwork in the tropics (Schultz 1971; Washburn 1977). Schultz, the greatest pre-war authority on primate anatomy whose fabulous collections of specimens, often obtained through expeditions, had earned him the nickname of ‘the monkey undertaker of Baltimore’ (Erikson 1981; Haraway 1989: 205), recalled:

For field work a lot of time is saved today with modern transportation by jet-plane and land rover car instead of by some slow tramp-steamer, dug-out canoe or on foot, as on my first expeditions. In those adventurous early days no noteworthy financial support could be obtained for such supposedly wild schemes, so we had to live off the land on bananas, monkey liver, iguana tails and armadillos, roasted in the shell, but we managed to bring back a lot of useful material and data, besides malaria. (Schultz 1971: 14)

But more than these technical improvements, a changing view on ‘primitive societies’ was the largest incentive for seriously starting to study the nonhuman primates from a human evolutionary perspective. This change did not only took place in anthropological circles, but belonged to a much wider reconceptualization of the nature of humanity in the Western world. In the bitter aftermath

of the Second World War when the devastating extent of the holocaust started to be revealed, racist theories based on intrinsic hierarchies from savage to civilized became hard to uphold. Auschwitz, Birkenau and Treblinka made painfully clear to which consequences such view of humanity could lead. If in the Unesco age following the war humanity was no longer made up of superior and inferior races (Haraway 1989: 197-203), it was hard to uphold that one group of humans could be seen as representative of an ancestral stage of the other (Cartmill 1990; Sperling 1991). Inferior races turned into non-western cultures, savage hordes became tribal bands, and primitive art evolved into high culture.

In anthropology, this changing perception is nowhere better illustrated than in the words of Ernest Hooton, the single-most influential American physical anthropologist of the 1940s (Haraway 1989: 204-5). Although much of his pre-war work was unabashedly racist, in a remarkable paper published shortly after his death he introduced a non-evolutionary perspective on foragers into this racial discourse: ‘Contemporary savages,’ he declared, ‘are not “primitive,” not on the evolutionary upgrade, not the stuff of which societal progress is made’:

I shall no doubt evoke the indignant disagreement of social anthropologists when I suggest that more is to be learned about the genesis of the human family and the beginning of social organization and community life in early man by the study of contemporary infra-human primates living under natural conditions than by the studies of retarded human groups living today under conditions variously described as “primitive,” “uncivilized” or “savage.” (Hooton 1955: 7-8)

In the nineteenth century human foragers had been savage enough; after the war they were no longer so. This meant that another source for imagining the human past needed to be tapped. To Hooton, primates—and primates only—could provide clues about human evolution. He was sided by the prehistorian Slotkin who believed it was logically and empirically invalid ‘to use generalizations from modern man [...] in interpreting the prehistory of other hominid forms’ and called for ‘a comparative study of the relevant processes in [...] the higher primates’ (1952: 442). According to Hooton, Carpenter’s field studies were far more instructive on human evolution ‘than any of the stuff on present-day savages written by anthropologists’ (8). The halt in primate studies caused by the war was therefore much to be regretted:

I know of no studies bearing upon man’s cultural and social origins that have been begun so brilliantly, that have progressed so magnificently, and that have been abandoned so miserably and ignominiously. It is as if one were to dig for oil, strike a gusher, and then immediately cap it up and go away and forget about it. Anthropology needs more studies of primate sociology. (Hooton 1955: 8)

Just like oil was the quintessential resource for modern North-American economics during the emergent Cold War, the nonhuman primate was the contested, but crucial fuel for Western speculation about humanity in the postwar world. Hooton’s words did not fall on deaf ears: one of his former students, Sherwood

Washburn, would initiate the practical implementation of that suggestion and his work would become the cornerstone of postwar American primatology (Haraway 1989: 204-223).

Yet Hooton's paper was also essential in another respect, for it defined the very term of 'primatology'. Instead of 'psychobiology' (Yerkes) or 'mammalian sociology' (Zuckerman), Hooton (1955: 2) declared:

I shall fall back upon the use of the word "primatology." This term, although repellent and linguistically a hybrid, is redeemed if not legitimated by a dual connotation: the study of the highest animal order and the study which of all the -ologies is, or ought to be, primus, prime, or first in importance.¹⁰

It is no coincidence that the term primatology occurs in the same paper which called for more primate studies to understand the process of human evolution.¹¹ Postwar primatology, particularly in America, was first of all motivated by an interest in australopithecines rather than in apes. Had psychology been the originator of primate studies in the prewar era, physical anthropology now took over the banner. The interest in the human psyche became an interest in human evolution. A broadly felt change in the postwar definition of humanity, 'physical anthropology's 1950s relocation of discourse about primitivity onto monkeys and apes' (Haraway 1989: 229) and the call of an evolutionary inspired science of primates under the flagship of 'primatology', these were the ingredients out of which the first primate model for human behavioural evolution would be distilled.

Washburn started his career as a Harvard physical anthropologist interested in the functional anatomy of the primates and it was in that quality that he had participated in the legendary Asiatic Primate Expedition of 1937 where Schultz shot gibbons for dissection and Carpenter observed them for publication. Washburn assisted Schultz with preparing the specimens and though he helped Carpenter for a couple of weeks, he later recalled that 'it was just the wrong time to expect me to shift gears from anatomy to behavior' (in DeVore 1992: 414-5). Instead, in subsequent years he worked hard to integrate the new evolutionary synthesis which had been forged by Dobzhansky and Simpson into physical anthropology (Washburn 1950; 1951). Sheer biometrical measurements could no longer do to explain primate anatomy as long as there was no genetic understanding of morphological adaptations. Most importantly, Washburn urged to replace the old concept of race by the new concept of population. This notion had been central in the new evolutionary synthesis of genetics and Darwinism but Washburn drew its implications for the biological study of humanity. To him, races were no longer fixed categories but formed a continuum that had to be understood in terms of relative degrees of gene frequency; differences between them were to be explained in terms of

10 It was a timely neologism which met with very little resistance among a new generation of researchers. Only a pre-war authority like Zuckerman (1981: 347) could find it a term which reflected 'hazardous specialization'—a justified warning since all too often primates were studied as if they were no part of the animal kingdom, but special organisms which demanded a different approach (Richard 1981).

11 Slotkin's (1952: 443) call to study primates instead of primitives used the term 'comparative primatology', indicating that the word primatology gained some acceptance in the first half of the 1950s.

'migration, drift, and selection' (Washburn 1951: 299). He wrote emphatically: 'Races must be based on the study of populations. [...] It is not enough to state that races should be based on genetic traits: races which can not be reconciled with genetics should be removed from consideration' (Washburn 1951: 299). Washburn was thus not simply influenced by the ideological climate of the postwar years, but actively contributed to the disqualification of racial hierarchies and their replacement by a more unified concept of humanity.

Motivated by Hooton and convinced that races were only populations, Washburn started to think about the nonhuman primates as sources to learn about the evolution of human behaviour. In the mid-fifties he steered his research in a more behavioural direction. With hindsight, he has often attributed this change to a lucky coincidence at the 1955 Pan-African Congress in Livingstone:

After the Congress [...] I arranged for a small collection of baboons. But much more importantly, as it turned out, there were troops of baboons close to the Victoria Falls Hotel where I was staying. The supply of baboons was irregular, and I spent any extra time watching the local troops. This was so much more rewarding that I closed out the collecting and spent my time watching the tame baboons' (Washburn 1983: 16).

From then on, he went to the nearby Wankie Game reserve and began 'just looking at baboons instead of dissecting them' (in DeVore 1992: 420). At the conference, Dart had presented his view on the osteodontokeratic culture but Washburn wondered whether the differential body preservation was really to be accounted for in cultural terms and began to study baboon behaviour (Washburn 1957). Together with his research student Irven DeVore he observed troops of baboons in Southern Rhodesia (now Zimbabwe) and Kenya because he believed this could enhance many of the questions concerning human evolution; the results of this research programme will be discussed in the next section. The influence of people like Hooton and Washburn can hardly be over-estimated. Hooton produced most of the doctorates in physical anthropology before the war and his students would take central positions in American academe, Washburn being one of them (Shapiro 1981; Spencer 1981). Of the first 19 doctoral degrees in modern primatology, 15 were supervised by Washburn (Gilmore 1981; Haraway 1989: 218, 222-3). The intellectual pedigree runs from Hooton to Washburn to DeVore to most of the practising primatologists in America nowadays (Loy and Peters 1991).

All ingredients for the preparation of a primate model were now in place. The final thing which was needed, was a feeling, if the culinary metaphor may be further stretched, of hunger, i.e. the experience of an intellectual *creux*, a conceptual void, a hiatus in our understanding or imaginative reconstruction. Early hominids from South-Africa had already whetted Hooton's appetite, but the further discovery of fossils by Louis and Mary Leakey in the Plio-Pleistocene layers of Olduvai Gorge in Tanzania brought water to the mouth. They not only ripped Dart's controversial Taung child out of isolation, but begged for an imaginative interpretation, being more ancient, more apelike and more ancestral than anything hitherto seen. The crucial find was that of 'OH 5' which Mary Leakey un-

earthed in 1959. How was this enigmatic skull to be interpreted? To what sort of being had it belonged? How were we to visualize the species it represented? In a first attempt of coming to terms with this well-known case of ‘Leakey’s luck’, the fossil was swamped with more names than any other fossil had hitherto received: Zinj, Dear Boy, Nut-cracking Man, *Zinjanthropus*, *Australopithecus boisei*, etcetera. Even today, there is no fossil with such elaborate birth certificate as this one. But name-giving, of course, was just a beginning.

The energetic and charismatic Louis Leakey believed that a better knowledge of extant great apes could help to clarify the fossils and living floors of early hominids. The story has often been told of how he sent three young women with minor ethological experiences and academic credentials to different species of great apes (Goodall 1971; 1990; Fossey 1983; Galdikas 1995; see also Cole 1975: 324–50; Kevles 1976; and particularly Morell 1995). Jane Goodall started her groundbreaking fieldwork at Gombe in 1960, Dian Fossey was sent to the mountain gorillas in Rwanda and Congo in 1966 and Biruté Galdikas has been studying Bornean orang-utans since 1971. Goodall’s long-term observations at Gombe revealed many unexpected behavioural patterns in the chimpanzee repertoire such as tool-use, meat-eating and hunting (Goodall 1986).¹² Along with the biomolecular demonstration of a close affinity between humans and chimpanzees, this research opened the door for many speculations on human evolution. Due to his discovery of ‘fossil men’ and his team of young women, Leakey was a much more seminal figure in the history of primate modelling than commonly acknowledged.

Both Washburn and Leakey were key-figures in the re-awakening anthropological interest in primate behaviour after the war. Both were driven by a desire to gain a better understanding of human evolution. Their programme and that of their respective students DeVore and Goodall gave directly or indirectly rise to the two main competing primate models for human evolution, the baboon model and the chimpanzee model, which will be discussed in the subsequent sections. In this sense, modern primatology was at its origin the answer to a palaeoanthropological question. This is certainly true for American primatology which, due to the intellectual legacy of Hooton, rose as part of anthropology—with the important consequence that apes and monkeys were studied separately from other organisms (Richard 1981). It is less true for Europe where primatology emerged within the classical ethological tradition as inaugurated by Niko Tinbergen and Konrad Lorenz with its focus on instinctive behaviour in jackdaws, geese

12 Tool-use by nonhuman primates was already well documented before World War II. Sixteenth- and seventeenth-century Jesuits had described chimpanzee tool use in the wild (Sept and Brooks 1994). Lubbock knew that ‘monkeys use stones to break nuts’ (1870: 481). Darwin had seen a captive orang-utan pounding the ground with a stone (Loy 1997). Köhler had already noted in the 1920s that a laboratory chimpanzee ‘will lick up ants, or hold out a straw for the ants to crawl on and lick them off’ (Kroeber 1928: 336, cf. Beck 1975). All this knowledge seems to have slipped through the mazes of history; or else it was forgotten in the heroizing of Jane Goodall. Contrary to a wide-spread belief, Goodall was not the first to describe chimpanzee tool use such as termite-fishing in the wild, she was the first to do so since the inauguration of primatology after the Second World War.

and sticklebacks (Lockard 1971; Roëll 1996; Fedigan and Strum 1997).¹³ Desmond Morris (1967b: 6) rightly argued that British and continental primatologists ‘approached monkeys and apes from the humbler side of the evolutionary scale, looking up at them from simpler, less brainy species, rather than down from the dizzy behavioural heights of man.’ Tinbergen founded an influential research school when he was in Oxford and supervised individuals like Robert Hinde who supervised on his turn Thelma Rowell, a forest baboon specialist. To them, the study of primates was a legitimate end in itself, irrespective of any anthropological implications; monkeys and apes were part of the animal kingdom, not hominids *manqués*. A similar motivation was also present in the work of the Swiss zoologist Hans Kummer on hamadryas baboons (Kummer 1995). Yet for better or worse, it turned out that those who worked on great apes, particularly chimpanzees, still got involved with issues of human evolution. Desmond Morris, Adriaan Kortlandt, Vernon Reynolds and later also Jane Goodall (who worked with Leakey in East-Africa but was supervised by Hinde in Cambridge) contributed to an understanding of human evolution, though never through the emphasis of referential model-building which typified American primatology. (Goodall’s data gave indirectly rise to the chimpanzee model, but this was, as we will see, in first instance an invention of American feminist anthropology, not hers.)¹⁴

The idea of primate modelling was a recent invention with a long prehistory in medicine, zoology, psychology, and finally physical anthropology. Primates had been used to tell something about modern human anatomy (Galen) and fossil human anatomy (Darwin), but the relevance of their behaviour for human social evolution was only explored after the Second World War. Why did it take so long? A few factors can be indicated.

Converging circumstances

One of the key factors in the rise of primate modelling was the establishment of evolutionary thought. In order to see nonhuman primates as instructive about our own ancestors, one was of course greatly helped by the suggestion that we descended from the same stock (Morris 1967b). Whether the precise mechanism of that evolution was Darwinian natural selection or Lamarckian inheritance of acquired characteristics was perhaps less important; what mattered was the acceptance of a simian descent of humanity. And this was largely established through the Darwinist revolution in the last quarter of the nineteenth century.

The idea of evolution alone, however, was not a sufficient cause; it took nearly a century between the Darwinian revolution and the appearance of the first behavioural model based on baboons. One reason for this delay was the lack of reli-

13 The Dutch ethologist J.A.R.A.M. van Hooff, one of Tinbergen’s students at Oxford, recalled in a recent interview: ‘I wanted to study primate behaviour but Tinbergen answered he knew nothing of mammals. He found them too complex. He literally said: “If it hasn’t got fins or feathers, I know nothing about it”’ (in *BIOnieuws*, 8 November 1997: 4, my translation). It was Desmond Morris who eventually brought Tinbergen in contact with ape research (cf. Van Hooff, forthcoming).

14 This distinction between postwar primatology in America and Europe echoes to a certain extent the earlier discussion with Yerkes’ believing in the human relevance of primate studies and Zuckerman’s abhorring such an idea.

ble evidence on primate behaviour as the first field trips started only in the 1930s. Basic difficulties of access and observation inhibited for a long time fieldwork in the tropics and it is true that postwar primatology (and model-building) benefited from many technical innovations in tropical medicine and transport. No matter how real these problems were, they do not adequately explain the empirical delay as they could all have been overcome in the nineteenth century. In fact, they were overcome at the time: travellers did penetrate into 'the heart of darkness' and if political and economic incentives were at stake the jungle could be crossed, if never easily. The adventurer Paul du Chaillu succeeded in tracking gorillas to shoot them, so why would it have been impossible to observe them? Fear certainly played its part. In the case of Du Chaillu, no sooner had he spotted a group of gorillas, that he pulled the trigger. Du Chaillu observed primates through the barrel of his gun; Garner through the bars of his iron cage. Yet the mixture of fear and fascination was no different for great apes than for 'head-hunters'—and they were elaborately described by contemporaries.

If technical difficulties and collective fear do not fully account for the tardy appearance of fieldwork, what else does? Perhaps it was not the brutish image of primates which prevented fieldwork but the brutish image of *primitives* which made it redundant. As said before, to the nineteenth-century evolutionist the savages were savage enough. One did not need to be a strong racist to believe that sub-Saharan Africans were on a much lower scale than Europeans, that they were bestial and that they, therefore, could serve as stand-ins for prehuman ancestors. Perhaps it was the case that primates *could* not be studied, but they certainly did not *need* to be studied with all this reliable information on human savagery around. Even in zoos, where research facilities were easy and safe, only very few studies on primate behaviour (apart from Darwin's study of facial expressions) were undertaken before Zuckerman in the 1930s.

It was only after the Second World War that one could no longer fall back on modern foragers, as clearly expressed by the words of even a racist thinker like Hooton and by the progressive disappearance of epithets such as 'savage' and 'primitive'. On top of that, the fossils which were now being unearthed were so much older and more ape-like that they made ethnographic parallels rather uninteresting. Compared to the Neanderthal or Java man, the primitive skulls like Taung (1925), *Zinjanthropus* (1959) and *Homo habilis* (1964) nearly begged to be compared with nonhuman primates, not just for anatomical study but also for behavioural reconstruction. Once radiometric dating of sediments unveiled surprisingly high ages of 1.8 Myr and older, it became necessary to consider that primates as more promising sources of inspiration than foragers. In the postwar world, 'early man' was 'less like modern man gone wild than like a primate tamed' (Fox 1975: 11).

It was this convergence of very ancient and ape-like fossils with a changing perception of human foragers and the continuing prewar belief that apes were revealing of the human psyche which gave rise to the first interest in primate models in the mid-1950s. The idea of primate-modelling was not born out of geniality but out of necessity. It was a relatively obvious solution for the sociocultural and

empirical problem human evolutionists and physical anthropologists faced in the 1950s. The method had proven successful in anatomy; now the challenge was to apply it to behaviour.

Baboons

Brief reviews of the history of primate modelling usually reduce the succession of models based on baboons, social carnivores, geladas, chimpanzees and bonobos to an antagonistic confrontation between advocates of the baboon model and the chimpanzee model (e.g. Reynolds 1976: 66-72; Tanner 1981: 19-22; Fedigan 1986; Kinzey 1987: xii; Foley 1992; McGrew 1992: 199).¹⁵ The former are said to argue from an ecological analogy: since baboons live in a savannah-like environment similar to the one inhabited by early hominids, their behavioural patterns must be alike. The latter are said to base their inferences on a phylogenetic homology: since chimpanzees and humans are genetically very close, behavioural similarities are due to common descent (homology) rather than convergent adaptation to a similar environment (analogy). According to this summary view of the debate, both models are responsible for two quite different images of the early human past: the baboon model stressed the role of male aggression and dominance in human evolution, epitomized by the Man the Hunter scenario; whereas the chimpanzee model unveiled a much more peaceful picture of primate social life focusing on the mother-offspring bond and the economic centrality of females, thus giving rise to Woman the Gatherer. Rather sweepingly, the baboon model has also been associated with the postwar pessimism on human nature, whereas the popularity of the chimpanzee model would reflect the flower-power idealism and second-wave feminism of the early seventies.

Although this textbook view is essentially true, it is a rather simplified and therefore unsatisfying version of the historical reality. Simplifying is the task of a textbook, but the following sections will attempt to do more justice to that reality by showing some of its complexities and by dissecting the structure of the analogical arguments put forward. Despite the number of reviews of the debate on primate models (in fact, the parade of baboons, social carnivores, geladas and chimps has become part of the undergraduate curriculum in physical anthropology), an analysis of their logical structure is still lacking (Fedigan 1986: 45). I will respect the divisions of this procession in that the sections focus each on a specific model and are more or less chronologically arranged. As in the two previous chapters my interest is first in ‘how’ the historical inferences are made rather than in ‘what’ they conclude, although here too the substantial side of the arguments will not be entirely avoided.

But to return to the baboons. Critiques of the model have nearly always assumed that the savannah parallel laid at its root. ‘Ecological similarity is thus the basis for the analogy,’ wrote Linda M. Fedigan (1982: 309) in her influential review of primate models. Likewise, Susan Sperling (1991: 9) argued that ‘the “baboonization” of early human life in such models rested on a savanna ecological analogy’. What was the result of this analogical reasoning according to its later

15 Fedigan (1982: 307-21) and Lewin (1984: 71-5) provide more detailed accounts of this debate.

critiques? ‘The themes that emerged directly from the baboon model,’ Strum and Mitchell (1987: 88) recount, ‘included: the adaptive nature of aggression, its use and control through a male dominance hierarchy, the differences in roles between males and females, and the relationship between aggression, dominance and reproductive success.’ By moving into an open environment with multiple predators, baboons and consequently early hominids had been forced to develop a rigid, hierarchical social structure based on dominant males who protected the group against outside predators and competed within the group for access to fertile females. The more dominant and aggressive an adult male baboon was, the more mating opportunities and control over females he received.

This rather grim-looking portrayal of early human sociality is in many ways a caricature view of earlier theories from the 1960s. It would be an exaggeration to state that it was just a straw man erected to be knocked down in order to make the chimpanzee model more plausible, but the truth is that both tenets of this account (i.e. the ecological analogy and the centrality of male aggression) owe more to popularizing books and textbooks of the late sixties and early seventies than to the original articles of the baboon specialists published before. More than anything else, it is the work of authors like Ardrey (1966; 1970), Morris (1967a), Tiger and Fox (Tiger 1969; Tiger and Fox 1971) that must be held responsible for the gloomy parallel between baboons and modern humans. Only there, the baboon was ‘the most aggressive of subhuman primates’ whose ‘ways are so uncomfortably reminiscent of man’s’ (Ardrey 1966: 228). It was only in general textbooks published some years later that baboons were used as a model for the past on the basis of an ecological analogy (Campbell 1967; Birdsell 1972; Pfeiffer 1972).

Authors like Ardrey, Morris, Tiger, Fox, Birdsell, Campbell and Pfeiffer had undertaken little or no fieldwork with baboons. They browsed through the publications of Washburn, DeVore and Hall to construe their arguments. Ardrey, for example, regarded Washburn ‘as our greatest anthropologist’ (1970:16) and found in Hall a ‘friend and counselor and drinking companion’ (Ardrey 1966: n.p.). Although baboons were equally being observed by others like Kummer (1968), Rowell (1966) and the Altmanns (1970), their work received far less attention from the science writers—partly because it did not draw implications about human evolution, partly because it offered a more moderate image of baboons. Rowell’s forest baboons did not defend the troop against predators but fled away in the trees; Kummer’s hamadryas baboons had no male dominance hierarchy but stable one-male units instead. Washburn, on the other hand, did explicitly speculate on the origin of man, and DeVore and Hall’s seminal descriptions of baboon social life focused on aggression and male dominance (DeVore and Hall 1965; Hall and DeVore 1965).

Washburn’s baboons: from typical primates to terrestrial specialists

In a number of articles published between 1958 and 1968 Washburn, each time accompanied by a second author (once Avis, twice DeVore), developed his ideas about the use of nonhuman primates in evolutionary research. These were the very first attempts at using extant primates in the reconstruction of early hominid

behaviour. A close reading shows how little of his arguments were based on an ecological analogy. He even never used the term ‘model’ when talking about the relevance of baboons, nor did he transfer notions of male aggression and dominance to early hominids. In fact, there was considerable interest for themes which modern commentators would more rapidly associate with chimpanzee research in the 1970s, such as play, mother-child relations and the almost entirely vegetarian diet of the baboon. These findings are so different from the received view of Washburn, as promulgated by his epigones, that the original texts deserve further inspection.

The quest for differences

In the introduction of their 1958 paper ‘Evolution of human behavior’, Sherwood Washburn and Virginia Avis realized that the problem of human evolution had been ‘greatly clarified by recent fossil discoveries and new techniques of dating and analysis’ (Washburn and Avis 1958: 421). The australopithecine finds from Africa shed important new light on human evolution, but a genuine interpretation of ‘the origin of human nature’ could be undertaken ‘only with the aid of imaginative reconstruction’ (422).

How was such ‘imaginative reconstruction’ to be done? In the line of what Hooton had predicted, the authors believed that an understanding of nonhuman primates could ‘enrich our interpretations of the bones’ (422):

much of human behaviour is shared with many other primates, and a general view of the origin of human nature from an evolutionary point of view must describe these features held in common as well as those which differentiate man.
(421)

Yet since the similarities between humans and simians had been repeatedly described, Washburn and Avis choose to emphasize instead ‘the origin of those features *differentiating* man from ape’ (421, italics added). Their devise was straightforward: look at humans and primates, list the differences between them and explain these in terms of human adaptations. To them, nonhuman primates functioned as stationary creatures from which the human branch had sprouted; they believed indeed that ‘many of the living forms [of nonhuman primates] have changed far less than has man’ (422). Human evolution had started there where nonhuman primate evolution had stopped. In the run towards humanity, extant primates provided the starting point, ‘modern man’ the end point, and the differences between them the stakes of hominization. The assumption that the nonhuman primate was more conservative than the human primate lay at the base of their (and many others’) ‘imaginative reconstruction’.

The body of their paper consisted of three parts: a comparison between monkeys, apes and ‘man’, a discussion of australopithecine fossils and a tentative sequence of the evolution of human behaviour. The first section listed the available evidence, even if it was ‘of very different caliber’ (421), on themes like reproduction and growth, social group, special senses, brain, locomotion, posture, and

movement. The information, neatly arranged in three columns for monkeys, apes and ‘man’, was largely anatomical since little was known about behaviour. Most evidence came from prewar authorities like Yerkes, Zuckerman, Hooton and Schultz; the only field study referred to was Carpenter’s 1940 work on the gibbon. As a result of this comparison, bipedalism, tool use, large brain size and language stood out as ‘the factors differentiating between the human way of life and that of the ape’ (421). They accounted for the appearance of unique human traits such as slow maturation, increased infant dependency, canine reduction, large territory and male provisioning. Hunting was not mentioned in the tables, but the authors believed that ‘man is clearly distinguished from all living monkeys and apes by being much more carnivorous, hunting and killing large animals’ (432).

The next step was to determine when these crucial attainments had been reached in the course of human evolution. The finds of australopithecines in South-Africa by Dart, Broom and Robinson provided a good starting point (Tobias 1985). Washburn’s study of the pelvis convinced him ‘that these can only be reconstructed as belonging to a bipedal animal’ (429). Repeating an old argument already suggested by Darwin, they believed that considerable reduction in australopithecine canine teeth in comparison to monkeys and apes made it ‘quite probable that they were using tools’ (430).¹⁶ Australopithecine brain size was still very small, which explained the absence of ‘tool making according to defined traditions’ (432). Did the australopithecines hunt? This question could only be answered by archaeological evidence but Washburn and Avis said: ‘Our belief is that the australopithecines were mainly vegetarian but had begun to supplement their diet with more animal food than is characteristic for the apes’ (433). Full hunting only developed with the emergence of large-brained creatures of the Middle Pleistocene, as represented by the fossils of Trinil and Zhoukoudian.

The resulting evolutionary narrative consisted of two stages. With *Australopithecus* we had a bipedal, small-brained, tool-using and largely vegetarian hominid which had moved into the savannahs, whereas increased brain size, complex technology, articulate speech and hunting of large mammals only developed with the hominids of the Middle Pleistocene which we would now call *Homo erectus*. The motor behind this drastic transition was hunting. Washburn and Avis described how hunting ‘had three important effects on human behaviour and human nature: psychological, social, and territorial’ (433). With hunting, man started to ‘enjoy the chase and the kill’; he became also economically responsible through sharing the prey in the group while his territory was greatly increased. Hunting was the key behaviour adaptation in human evolution;

16 Washburn and Avis are rather ambiguous about the function of early tools. In one place, australopithecine tools are said to be defensive weapons (430); in another, they might have been part of the hunting equipment (432); still further, they were ‘probably used to extend the quantity and variety of this [vegetable food] rather than to obtain meat’ (433). Tools could have been used for fighting, hunting or ‘digging, crushing, and tearing things open’ (433)—the latter option would become the feminists’ favourite (cf. infra).

hunting was what made us human.¹⁷ This is to be taken quite literally: in the article the australopithecines are still referred to as ‘animals’ or ‘forms’, whereas the hunters of the Middle Pleistocene are called ‘men’. Human evolution was thus ‘the change from some animal such as *Australopithecus* to the primitive men of the middle Pleistocene’ (432).

What was the role of the nonhuman primates in this article? Clearly, no single species was cast as a model for human evolution or for any specific stage of their evolutionary reconstruction. In fact, the nonhuman primates preceded the australopithecines at the start of human evolution, but even then not as abstract prototypes. ‘The search for the unspecialized common ancestor,’ Washburn (1950: 69–70) had written before, ‘becomes either a denial of evolution or a hunt for an illusory, philosophical archetype.’ Washburn and Avis looked at modern primates (or what was known about them) to detect the differences with humans. The process of evolution could be known by the subtraction of modern humans minus modern nonhuman primates. Monkeys and apes on the one hand and humans on the other served as two clothes-peg between which the line of hominization was hung. In this sense, their method did not proceed by analogy but resembled the programme set forward by an influential paper published a couple years before by the zoologists Bartholomew and Birdsell. In order to study the ecology of the ‘protohominids’, they urged ‘to extrapolate upward from ecological data on other mammals and suggest the biological attributes of the protohominids and to extrapolate downward from ethnological data on hunting and collecting peoples and suggest the minimal cultural attributes of the protohominids’ (Bartholomew and Birdsell 1953: 481). This ‘sandwich’ approach to human evolution would remain a popular alternative to referential models.

The only hint of referential modelling in Washburn’s first paper came from a surprising angle: carnivores. The pleasure in hunting other animals was ‘strikingly similar to that of many carnivores, and no parallel behavior has been observed among wild primates. [...] If one watches baboons in one of the great African game reserves, one sees that they move unconcernedly among a great variety of animals, and it is only when large carnivores appear that the animals react [by fighting and fleeing] as they do to man’ (433). Although Washburn had already observed baboons in Africa, by the time this paper was written, he saw them more as apprehensive than representative of humans. Social carnivores, on the other

17 So much has been written about the centrality of hunting in evolutionary scenarios of the 1950s and 60s that it has become part of the standard history of anthropology (see Perper and Schrire 1977; Haraway 1989: 206–17; and especially Cartmill 1993: 1–27). From Dart’s speculations in the 1950s about the belligerent and bloodthirsty nature of the australopithecines, over Ardrey’s dramatic popularizations of this theory (Ardrey 1961; 1976) to the extreme contributions of the Man the Hunter conference (Washburn and Lancaster 1968; Laughlin 1968), the hunting hypothesis has been criticized since the 1970s. The famous Man the Hunter conference, held in Chicago in 1966, signalled not so much the start of the hunting hypothesis but its vociferous apex. The papers by Laughlin, Washburn and Lancaster in the resulting volume (Lee and DeVore 1968) were the most explicit defences of the idea that hunting had made us human, but the volume also contained the germs for a refutation of this idea, notably in the contribution by one of the co-editors: Lee’s study on the !Kung San showed that hunting played only a minor role in the subsistence economy of these supposedly archetypical hunter-gatherers. Lee would later call the volume ‘mistitled’ (Lee 1979: 16).

hand, resembled humans in the ‘economic responsibility of the adult males and the practice of sharing food in the group’ (434). Consequently, ‘the very same actions which caused man to be feared by other animals led to more cooperation, food sharing, and economic dependence within the group’ (434). Aggression, only mentioned once in the paper, was not inherent to the primate repertoire but originated with the hunting habit: ‘carnivorous curiosity and aggression have been added to the inquisitiveness and dominance striving of the ape’ (434).

Contrary to the received view, in this early Washburn paper primates did not serve as a model which injected doses of aggression, hierarchy and sexual politics into our view of human origins. Hunting, often seen as the result of the baboon model, was already Washburn’s major explanatory principle well before ‘studies of groups of living primates emerged as a major capstone to his academic edifice’ (Haraway 1989: 217-8). It served to account for the more recent changes in human evolution (not the whole process of hominization), and it was more readily associated with a carnivore model (not a baboon model).

Baboons at the lower limit

Three years after the ‘Evolution of human behavior’ appeared, Washburn published an article together with his graduate student Irven DeVore (Washburn and DeVore 1961). In the meantime, both had been doing fieldwork with olive baboons in national parks in Southern Rhodesia (now Zimbabwe) and Kenya, observations which had taught them more than that baboons ran away from humans and other carnivores (this work formed the core of DeVore’s Ph.D. thesis from 1962 on the social organization of baboons). In 1961 the time was ripe ‘to present a brief account of the daily life of baboons to serve as a background for the discussion of the social life of early man’ (91) and this was exactly what the ‘Social behavior of baboons and early man’ did.

At first sight, the differences with the 1958 paper could not have been greater. The sweeping statements on primates had turned into detailed descriptions of baboons; the emphasis on anatomy had changed into an exclusive attention to behaviour. Was the 1958 paper a theoretical speculation on human evolution, the 1961 publication looked like an ethnography of a primate society with only limited larger implications. Despite this initial impression, the paper shows profound similarities with earlier work; both the aim, method and conclusions closely resembled the previous article.

The ultimate aim of Washburn and his co-author consisted of interpreting the course of human evolution and ‘reconstructing the social life of early man’ (95). To this end, they used the same comparative reasoning as before. Focusing on themes like troop size, range, diet and population structure, the behaviour of the baboons was compared with ‘preagricultural humans’ (102) and with what was known about fossil hominids. Some general similarities were noted (e.g. in troop size) but in general the authors ‘tried to outline some of the ways in which the way of life of contemporary baboons differs from that of man’ (103). These differences, eventually summarized in a table at the end of their paper (figure 14), served to point out what was distinctively human. Unlike the baboons, humans

had a much larger territory with home bases, a carnivorous component to their diet, an avoidance of inbreeding, an extended mother-child bond, an increased economic dependency of the infant, a prolonged male-female relationship, a reduced dominance and the faculty of speech. These differences were to be interpreted in terms of human evolution: ‘because of the great behavioral gap between man and his nearest relatives, some reconstruction of behavior is possible’ (103). As in 1958, many of these patterns emerged as direct or indirect consequences to hunting. ‘The basis for most of these differences may lie in hunting,’ they wrote in a more popular article in *Scientific American* published the same year (1961b: 19). The Middle Pleistocene was again the turning point in human evolution: ‘By the Middle Pleistocene there is direct evidence of hunting and indirect evidence for co-operation, division of labor, and sharing of food. This human pattern differs on each of these points from that of baboons, who do not hunt, share, or co-operate, and where there is no sexual division of economic activity’ (98).

Their final conclusion recaptures the earlier ideas of Washburn and it is worth quoting their entire last paragraph:

We see two stages of behavioral evolution separating the apes from Homo sapiens. The first of these is that of the australopithecines of the Lower Pleistocene. Although these forms were bipedal and tool-making [contra 1958], there is little to suggest that their social life was very different from that of apes or monkeys. They were probably primarily vegetarian, and the small-brained young could have matured rapidly. Perhaps only the rudiments of the human way of life were present. But, by the Middle Pleistocene, large-brained men who hunted big animals were present, and this may well have been the period during which the distinctively human attitudes on hunting, territory, and the family originated. At least the biological and economic problems that ultimately led to the social customs of today had their roots in the hunting societies of half a million years ago. (103)

From the previous passage it is clear that baboons did not serve as referential models on the basis of ecological analogy. They were used to highlight, through contrast, a set of unique human traits. As little was known about the variability of primate behaviour, patterns of baboon behaviour were thought to be prototypical. The *Scientific American* audience could read that ‘baboon characteristics [...] may be taken as representative of ape and monkey behavior in general’ (Washburn and DeVore 1961b: 18-9). Washburn and DeVore believed that the main points of their comparison ‘would not be greatly changed by substituting other nonhuman primate species for baboons’ (1961a: 103). Baboon behaviour was prototypical primate behaviour, not an adaptation to a specific environment. The word ‘ecology’ did not even occur in the whole paper! To put it unceremoniously, in 1961 it did not even matter whether baboons lived in savannahs,

SUMMARY TABLE

	<i>Baboons.</i>	<i>Preagricultural Humans</i>
Group size . . .	10-200	50-60
Group density . .	10 baboons per square mile	5-10 square miles per individual
Range	3-6 square miles; no territorial defense	250-600 square miles; territorial rights, boundaries defended from strangers
Diet	Almost entirely vegetarian; no food-sharing or division of labor	Omnivorous; food-sharing; men specialize in hunting, women and children in gathering
Population structure . . .	Small inbreeding groups	Tribal organization with local exogamy
Social system . .	Self-sufficient, "closed" system; temporary subgroups based on preference and age	Interband affiliation and dependency, semiopen system; subgroups based on kinship
Play	Largely interpersonal and exploratory	Also interpersonal, but includes considerable play with inanimate objects
Mother-child relations . . .	Intense but brief; infant well developed and in partial control, abduction of toe, etc.	Long; infant helpless and entirely subject to attention of adults
Sexual behavior .	Female oestrus; multiple mates; no prolonged male-female relationships	Female continuously receptive; family structure based on prolonged male-female relations and incest tabus
Economic dependence .	Infant economically independent after weaning; no hunting, storage, or sharing of food; full maturity biologically delayed	Infant dependent upon adults for many years; hunting, storage, and sharing of food; male maturity both biologically and culturally delayed
Dominance . . .	Sexual dimorphism with large canines in males	Minimum sexual dimorphism, canines reduced; tools replace teeth in fighting and defense
Home base . . .	No base	Improved locations, which are occupied for periods of time
Sounds and gestures . . .	Species-specific communication; largely gestural and concerned with immediate situations	Linguistic community based on speech

Figure 14. Washburn and DeVore initially studied baboons as representative of a primate pattern prior to hominization. In this table, they juxtaposed evidence on baboons and preagricultural humans in order to indicate the extent of human uniqueness (Washburn and DeVore 1961: 102).

woodlands or forests.¹⁸ They were just a concrete example of the generic primate picture as outlined in 1958. Consequently, baboons were not living representatives for one of the two stages of the evolutionary scenario but preceded them as prototypical representatives of the simian substrate from which humanity had started. *Australopithecus* retained much of their rudimentary social life, whereas *Homo erectus* more closely resembled modern hunter-gatherers.

Not only were baboons absent as referential models, even the carnivores became more doubtful for the job. Though they shared food like human hunters, because of its greater communities ‘the human situation was far more complicated socially than that of other carnivores’ (100). As ‘no comparable situation exists among other carnivores’ (100), the authors refrained from looking for further modern analogues for this ‘new social problem’ (101). As such, the 1961 paper eradicated all referential modelling.

The portrait of the baboon that arose from Washburn and DeVore’s description is nearly angelical compared to its later caricature. Although ‘the readiness to eat meat is present’, their diet is ‘overwhelmingly vegetarian’ (94). Instead of aggression, ‘play is an important activity, which occupies many hours every day’ (96). Consort pairings are said to ‘last a matter of hours or days and usually dissolve peacefully,’ the odd fight notwithstanding (97). In general, however, ‘fighting within the troop seldom occurs, because the position of each animal is recognized’ (100). These quotes are not the result of selective reading; they just show how distorted the later reconstructions are.

When it came to Washburn and DeVore’s views on the human past, male hunting was indeed seen as more important than female economic activity, at least in diachronic perspective, i.e. in terms of its impact on human evolution.¹⁹ Later critics, however, have failed to notice how hunting ‘required that co-operation and sharing increase and that dominance become less important in social control’ (101). Instead of more, according to Washburn and DeVore, we got *less* dominance in human evolution as a consequence of hunting! Co-operation, food sharing, food transport, home bases, gathering—all these so-called later correctives to the hunting hypothesis are present in Washburn’s early work. Although the leading principle was that ‘man’s success comes from hunting’ (92), the connotations of aggression and dominance were *not* associated with this early version of the baboon model.

18 The work of Thelma Rowell (1966) would later demonstrate that the behaviour of forest-living baboons was quite different from the savannah baboons observed by Washburn and DeVore but it never received attention from a primate-modelling perspective, partly because ecological analogy became more important in the late 1960s (their forest habitat was thus considered irrelevant to the hominid savannah), partly because their behavioural patterns responded less to the notion of aggression that became prevalent in the late 1960s and early 70s. They thus lived in the wrong environments and performed the wrong behaviours.

19 In synchronic perspective, the importance of gathering and the vegetable component of the diet was widely acknowledged but this was thought to entail little evolutionary consequence. As Donna Haraway (1989: 217) poignantly observed: ‘Gathering is about local foods; hunting is about universal principles.’

Living australopithecines

If the 1961 article looked very different but turned out to be very similar to its predecessor, our next paper shows the exact opposite. Published by DeVore and Washburn in 1963, its extensive descriptions of baboon life looked very much like the previous publication, but the underlying rationale for studying baboons was quite different.

The article ‘Baboon ecology and human evolution’ opens with the observation that these animals are ‘the most successful ground-living primates, and their way of life gives some insight into the problems which confronted early man’ (335). The intention of the authors was no longer to present an account of human evolution (their two-staged view of the matter was presented in the introduction as a taken-for-granted with hunting as the key adaptation); now they were interested in ‘the problems which confronted early man’. Once a gradual ascent from the apes, human evolution has now all of a sudden become a problematic and contested struggle. The new word was ‘survival’, the new paradigm ‘ecology’, the new unit of analysis ‘the group’. The understanding of behaviour in terms of its environmental parameters became so dominant that studies on baboon social organization started to be called ‘baboon ecology’ (the phrase occurred in several titles; cf. DeVore and Washburn 1963; DeVore and Hall 1965 and Altmann and Altmann 1970).

Consequently, the baboon was no longer the spokesperson for the other 200-odd species of nonhuman primates. In the first pages of the article, DeVore and Washburn distinguished for the first time between different species of baboons and contrasted the genus *Papio* with other genera of monkeys, thus indicating the extent of behavioural variation. If baboons were specific instead of generalized what could further justify them as a source for analogy? The answer was simple: because they lived on the ground. Not because they lived in savannahs or because they practised hunting, but simply this: the habit of shuffling, running, feeding, playing, copulating, fighting and dying on the ground. ‘Aside from man, these monkeys are the most successful ground-living primates,’ DeVore and Washburn held (1963: 335). A terrestrial life-style inevitably led to a greater evolutionary flexibility, following the maxim: ‘the more ground-living, the less speciation’ (337). Since ‘the men of the Middle Pleistocene, genus *Homo*, occupied the same range as the baboon-macaques without speciation’ and since ‘*Australopithecus* may have occupied an adaptive position midway in effectiveness between the ground monkeys and early *Homo*’, the sequence from baboons to australopithecines to early *Homo* could be safeguarded as in 1961. Baboons were no longer representative of other nonhuman primates, but terrestriality and lack of speciation got them back in evolutionary reconstruction by the back door.

With this new emphasis on survival and terrestriality, the leading research question became: to what extent are behavioural and anatomical patterns the result of ‘adaptations to life on the ground’ (342) in a predator-rich environment? Many of the earlier observations were now re-interpreted in function of ecology and survival. Was play once considered to build ‘social skills’ in 1961 (96), now it gave males ‘fighting practice’ (347). Was the position of each animal recognized in the troop in 1961, two years later ‘interadult male antagonism’ reigned (347). Male dominance, sexual dimorphism and social hierarchy were all described in terms of terrestrial adaptations.

The most typical example of this new approach was DeVore and Washburn’s description of how baboons moved in the open, treeless plains: subdominant males and older juvenile males walked out in front of the troop and at its rear end; between them came pregnant and estrus females as well as juveniles; in the centre you found a nucleus of dominant males, females with infants and young juveniles. In case of predator attack, such arrangement insured ‘maximum protection for the infants and juveniles in the center of the troop’ (344). This interpretation epitomizes the ecological approach: social behaviour was seen as an adaptation to certain ecological pressures (predation), it was described in terms of age groups and sex classes, and it ultimately contributed to group survival. The study of the baboons’ marching order became paradigmatic for this new ecological approach to primatology and found its place in many textbooks—regardless of the fact that it had already been described as early as 1623.²⁰

In general, baboon behaviour was interpreted through a military and martial metaphor. The social group, significantly called the ‘troop’, had become the unit of evolution.²¹ Each troop disposed of a ‘home range’ with a ‘core area’, roamed by inimical predators. ‘Survival’ was its only purpose: ‘Sex differences, peripheral animals, and range—each of these has meaning only in terms of the survival,’ DeVore and Washburn argued (366). Its social structure resembled a military hierarchy with well prescribed functions based on ‘age, sex, personal preferences, and dominance’ (342): the less dominant adult males and juveniles defended the moving troop like soldiers in the firing line, whereas the dominant adult males protected the helpless mothers and young infants inside the troop. Food shortage and finding safe sleeping trees were the troop’s logistical problems. The alternation between resting under trees and moving in battle-array through exposed

20 Richard Jobson wrote: ‘But to speake of the Babowne, I must say, it is a wonderfull thing [...] as they travell, they goe in rancke, whereof the leaders are certaine of the greater sort, and there is as great, and large of them, as a Lyon, the smaller following, and ever now and then as a Commaunder a great one walkes; the females carry their yong under their bellies. [...] In the rear comes up a great company of the biggest sort, as a guard against any persuing enemy, and in this manner doe they march along’ (quoted in Yerkes and Yerkes 1929: 9).

21 The troop was seen as the level where selective pressures where at work, unlike more recent views which consider the individual or the gene as the unit of evolution (Gilmore 1981; Ghiglieri 1987). ‘The group is an adaptive unit, the actual form of which is determined by ecological pressures’ (Gartlan 1968: 115). DeVore (1965: ix) defined the troop as follows: ‘A “troop” so defined consists of a discrete group of adults of both sexes, together with juveniles and infants, that maintains social identity and spatial unity over long periods. Such a troop is an easily recognizable unit with relatively impermeable social boundaries, although young males or groups of young males may live outside the troop.’

country reminded of troop movements in war. Baboon social structure seemed like a well-oiled battle-machine in the land forces of the East-African savannah.

The resulting baboon image was considerably different from the previous paper by Washburn and DeVore and seems more familiar to us today. Baboons were ‘more aggressive and dominance-oriented’ (336) than other monkeys. Between a troop and its predators, between different troops, but mostly within the troop itself, aggression was the organizing principle and dominant males formed the pivot of the troop. ‘The role of adult male baboons as defenders of the troop [was] vital to the survival of the troop, and especially to the survival of the most helpless animals—females with new babies, small juveniles, and temporarily sick or injured individuals’ (346). Some years later, Hall and DeVore (1965: 54) began their study of baboon social organization with the axiomatic phrase: ‘The baboon group is organized around the dominance hierarchy of adult males.’ Male aggression, male dominance and male defence—these were the fixed parameters of baboon behaviour. The article of DeVore and Washburn contained a picture of a threatening adult male baboon which more than anything else summarized and disseminated this new view (figure 15).

But how about hunting? It is tempting to see DeVore’s and Washburn’s detailed descriptions of hunting episodes as another symptom of a change in their perception of baboons. In 1961, they spent only two sentences on baboon hunting (‘A small live animal in the grass will also be killed and eaten. We saw two newborn Thompson’s gazelle, two half-grown hares, and three nestlings of ground-nesting birds killed in this way’, p. 94); in 1963, these same episodes were good for more than four, juicy pages (‘An adult male baboon grabbed it, brought it above its head, and slammed it to the ground. He immediately tore into the stomach of the gazelle and began eating it.’ p. 360). These appetizing scenes notwithstanding, baboons were still described as ‘very inefficient predators’ (362) and their attitude towards other animals was ‘not that of a predator’ (363). Since ‘the importance of meat in the baboon diet has been considerably overstressed’, Washburn and DeVore found it more reasonable ‘to assume that meat has been a consistent but very minor part of the baboon diet throughout their evolutionary history’ (363). Hunting remained the human prerogative *par excellence*.

What role did the baboons finally play in the reconstruction of human evolution? DeVore and Washburn (365) were very clear about this:

Obviously, man is not descended from a baboon, and the behavior of our ancestors may have been very different from that of living baboons. But we think that in a general way the problems faced by the baboon troop may be very similar to those which confronted our ancestors. At the least, comparison of human behavior with that of baboons emphasizes differences. At the most, such a comparison may give new insights.

This dual employment can be clearly seen in the text. First, many passages remind of the ‘comparative’, difference-seeking method of the two previous papers. Baboons are thus said to have more closed societies, larger troop sizes and greater population densities in comparison with humans. ‘With the coming of



*Figure 15. Male dominance and aggression come together in this emblematic picture of a young adult male baboon displaying his canine teeth (DeVore and Washburn 1963: figure 6). This powerful image was reprinted in Campbell's undergraduate textbook on human evolution (1967: 274). Similar photographs appeared in Hall and DeVore's classic baboon study (Hall and DeVore 1965: 58-9), in Pfeiffer's handbook on human evolution (1972: 239) and in the popular Time volume on the primates (Eimerl and DeVore 1965: 59, 117). The latter picture even inspired the cover illustration for the Penguin edition of Marais' *The Soul of the Ape* (1969). The aggressive baboon image was thus widely disseminated over the Western world. If you hadn't observed baboons in Africa yourself, the message inherent in this illustration was clear.*

man, every category is fundamentally altered. [...] Some measure of how different the new directions are may be gained from the study of the ecology of baboons' (366). Next to this 'minimal' approach, the 'projective' method provides new insights by projecting baboon ecology back onto a specific period. Here, baboons no longer precede the known fossil species but start to represent certain known, particularly older stages. In a section on australopithecines, the authors write: 'It may be possible to reconstruct more of this stage in human evolution with a more thorough study of the ecology of baboons' (338). Their discussion on scavenging,

a subject ‘so important, especially in the interpretation of the deposits in which *Australopithecus* is found’ (364), explores this second approach.

It seemed reasonable that scavenging once was an important phase in human evolution, but observations in the field taught them the opposite: there were far fewer kills than expected (the lion-baboon ratio was 1:100); most kills by predators were made and consumed at night; if anything was left, lions would usually stay nearby the meat during the day; and baboons did not take the slightest interest in the rare cases they ran into a ‘safe’ kill (364–5). DeVore and Washburn concluded that their ‘opinion of the importance of scavenging has changed through observation of the actual situation at the kills’ (366). This exercise was the first use of baboons as genuine analogues for australopithecines.

Since baboons and australopithecines are both terrestrial animals (observed similarity), the absence of scavenging with the former could be extrapolated towards the latter (predicted similarity). But how strong was the claim that ‘the scavenging theory is not supported by the evidence’ (364)? The many differences between baboons and australopithecines (relevant dissimilarities), such as bipedalism and incipient tool-use, weakened the argument. There was no necessary link between ground-living and the absence of scavenging (weak relevance of similarity). On top of that, the observed similarity was vague (ground-living can be done in many environments). Observations took place in only two national parks (limited number and variety of source contexts), where the predator-baboon ratio might have been very different from past environments. The conclusion was not tested nor falsified. Considering the logical obstacles this first primate model ran into, it should not come as a surprise that australopithecine scavenging has been a bone of contention ever since.

In the papers written by Washburn and his co-authors between 1958 and 1963, we see three attempts to use nonhuman primates in the reconstruction of human evolution. Initially based on generalized statements about monkeys and apes, speculations on human evolution came to invoke contemporary baboons, first as prototypical primates (Strier 1994), later as terrestrial specialists. The baboon shifted from a lower limit of human evolution to a referential model for early australopithecines. In the first case, the method was comparative and looked for differences; in the second case, it was projective and looked for similarities. Parallel to the shift from a comparison to an analogy, the baboon took on the role of the malicious, malevolent, aggressive primate.

Before studying the consequences of this shift, we need to understand its causes. The early 1960s witnessed a tremendous increase in naturalistic field studies on monkey and apes (cf. the compilations by Buettner-Janusch 1962; DeVore and Lee 1963; DeVore 1965 and S. Altmann 1967), which certainly affected the above-sketched change in approach. In 1963, DeVore and Lee could already compile a list of nearly fifty contemporary field studies on nonhuman primates, on species ranging from tree shrews to gorillas (DeVore and Lee 1963). However, about half of the studies focused on macaques and baboons so that a more encompassing understanding of variability in primate behaviour beyond the cercopithecine realm was not yet realized (figure 16). If one wanted to undertake a

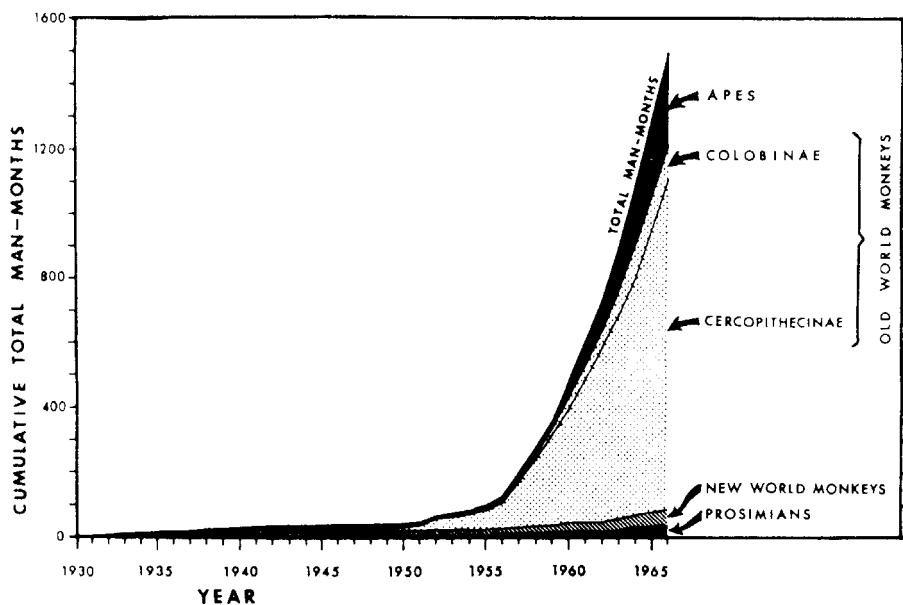


Figure 16. This graph, compiled by Stuart Altmann (1967: figure 1), shows how the increase in primate studies during the early 1960s was by and large limited to baboons and macaques, resulting in 'the myth of the typical primate' (Strier 1994)

comparative study on primate tool use, like Kortlandt and Kooij (1963) did, it was still necessary to rely on questionnaires sent to zookeepers and field biologists to document variability. More started to be known about primates but data did not accumulate so rapidly that it could overthrow Washburn's argument on the baboon's representativeness in just a few years time. On the contrary, 'concepts about *primate* society were, for many years, actually generalizations about *baboon* society' (Strum and Mitchell 1987: 89).

A much more important reason for the change in emphasis between 1961 and 1963 lies in the minor detail of authorship: in 1963, DeVore was the first author and he kept different ideas on the study and relevance of baboons.²² Washburn observed baboons largely irrespective of their environment; for DeVore this was untenable. In this, he was strongly influenced by the socioecological work of K.R.L. Hall whose 'notion of an adaptive relationship between social organization and environment rapidly became a focus for syntheses of primate field research, and the demonstration of this relationship in specific instances became the goal of many field studies' (Richard 1981: 518). Primate behaviour, i.e. the social structure of the group, could only be understood through primate ecology. The most influential overview was given by Crook and Gartlan (1966) in *Nature* who distinguished between five adaptive grades of primate behaviour, each one 'intimately linked to species ecology' (1200). Since the environment dictated behavioural adaptation, the terrestrial similarity between baboons and australopithecines was a relevant base for analogy: under a given ecology (understood as the conditions of

22 The 1963 paper was one of the rare in Washburn's career where he was not the first author, which really indicates the extent of DeVore's contribution to it.

food availability, food dispersion and predator pressure) only a very limited number of adaptations were possible, even in phylogenetically distant species such as baboons and australopithecines.

DeVore was responsible for this change in approach. When five years later Washburn, together with C.S. Lancaster, wrote his classical and often-quoted version of the hunting hypothesis for the *Man the Hunter* volume, he abstained from directly applying baboons to human prehistory (Washburn and Lancaster 1968). Although the baboon studies and the hunting hypothesis are often bracketed together—undoubtedly because of their mutual reliance on male dominance and aggression—the baboon model was not essential to it. Indeed, the hunting hypothesis existed already well before Washburn started looking at baboons and its largest impact was rather on the carnivore model. DeVore and Washburn called baboons ‘very inefficient predators’. The only references to extant nonhuman primates in Washburn’s *Man the Hunter* contribution paper were again of the sort of ‘general differences with man’, comparable to his earlier work. DeVore, on the other hand, continued to study baboons and published two standard articles together with Hall (DeVore and Hall 1965; Hall and DeVore 1965) but the ambition to shed light on human evolution was entirely absent here. By the mid-sixties baboons had become legitimate objects of study in their own. In the work of DeVore we see the emancipation of primatology as an autonomous discipline from its initial background in human evolution research (DeVore 1965).

The projective baboon model as it first emerged in the 1963 paper was thus the result of the two rather distinct preoccupations of its authors: Washburn’s ambition to shed light on human evolution and DeVore’s perspective on ecology as the key to understanding behaviour. It was this convergent mixture which would become the basis for later articulations of the baboon model and the target for all subsequent critiques. And, as so often with original and provocative work, it was lauded and despised at the same time.

Washburn returned to general statements, DeVore no longer drew implications on human evolution, Hall had never attempted to do this. The baboons were now in the hands of non-specialists.²³

The canonization of the baboon model

Imagine a lay person in the early 1970s who is interested in human biology and evolution and wants to know more about it. He would go to a library or a bookshop and pick up from the shelves a best-seller like Ardrey’s *Territorial Imperative* (1966) or Morris’ *Naked Ape* (1967a). If passionately devoted, he might even go through some of the recent undergraduate textbooks which made the scientific

23 A younger generation of baboon experts like the Altmanns and Rowell also studied baboons in their own right. The moment baboons started to be systematically studied, they were no longer staged as living ancestors by their observers. Just like Spencer and Gillen’s intensive fieldwork with the Arunta made clear at the turn of the century that foragers were quite complex and could not simply be used for evolutionist projection, the baboon studies of the late 1960s which documented the variability of primate behaviour problematized the straightforward use of these animals as palaeoanthropological models.

debate of the last decade accessible and comprehensible. In any case, our keen reader will trust that what he consults is an accurate, though simplified, reflection of the state of the art. This, however, was not the case.

To understand all subsequent debates on primate modelling requires to understand the role of textbooks and popular-science publishing in the early 1970s.

The dark side of the baboon

A number of books published in the late 1960s forged the observations of Washburn, DeVore and Hall into a grim caricature of baboon life. Take, for example, the image presented in the work of the former American playwright Robert Ardrey, whose dramatic prose always makes for good quotation:

The student of man, perturbed by the future of human warfare, by the apparently inviolate laws of territorial conflict, and by human reluctance to abandon the intruding way, may find the baboon the most instructive of species. Among primates his aggressiveness is second only to man's. He is a born bully, a born criminal, a born candidate for the hangman's noose. As compared with the gorilla—that gentle, inoffensive, submissive creature for whom a minimum of tyranny yields a maximum of results—the baboon represents nature's most lasting challenge to the police state. He is as submissive as a truck, as inoffensive as a bulldozer, as gentle as a power-driven lawnmower. He is ugly. He has the yellow-to-amber eyes that one associates with the riverboat gambles. He has predatory inclinations, and in certain seasons he enjoys nothing better than killing and devouring the newborn fawns of the delicate gazelle. And he will steal anything. (Ardrey 1966: 227)

Even if extreme, this statement is representative of the aggressive baboon image in popular science writing of the time. Lewis Binford, the vehement combatant of the New Archaeology, made the following sardonic remark in 1972: 'I am certain that my days of delivering papers at national meetings, where my behavior has been compared to that of a male baboon, huffing, puffing, and throwing eyelid threats in all directions, is not over' (1972a: 451). Ardrey believed that the baboon, being 'so unpleasantly reminiscent of man' (228), could shed light on the problems of man, and in particular human warfare—academic polemics apparently being one form of it.

A similar ambition to elucidate human nature by looking at other nonhuman primates characterized the work of Ardrey's British pendant, Desmond Morris. His book *The Naked Ape* (1967a) was significantly subtitled 'a zoologist's study of the human animal'. Just like Ardrey, he wrote for a broad audience, summarized primate field studies (though more on chimps), and gave biological explanations for our cultural habits, thus offering the world a number of easy stories on why women put on lipstick and walk on high heels. His lengthy, if clinical, descriptions of 'the naked ape's' sexual behaviour ('bouts of oral stimulation' for kissing; 'skin manipulation' for caressing) injected a fair dose of human eroticism in the acceptable format of zoological prose and made the book a genuine best-seller.²⁴

24 For a virulent critique on this practice, see Zuckerman (1981: 387-97).

Writing for a more specialized audience, Lionel Tiger and Robin Fox equally sought to bridge the gap between a cultural and a biological perspective on man. In a seminal article, published in the very first issue of *Man*, they called for ‘a marriage of the two disciplines’ of biology and sociology with the former, however, as the undisputed *pater familias*: ‘sociological findings, in this perspective, provide data for a more comprehensive, zoological approach’ (Tiger and Fox 1966: 22). With book titles as *Men in Groups* (Tiger 1969) and *The Imperial Animal* (Tiger and Fox 1971), it was inevitable that male-dominated baboons were invoked to supply the necessary primate evidence. Not so much because they were representative primates but because they ‘have spent a considerable portion of their evolution outside the forest environment’ (Fox 1968: 51). DeVore had defended baboons as models because they were *ground-living*; with Tiger and Fox it was because they were *savannah-living* (cf. Martin and Voorhies 1975: 135). This change was due to the increasing number of studies on baboons in different ecological environments. Rowell’s (1966) study of baboons in the forests of Uganda showed a less male-dominated form of primate society. Kummer’s (1968) study of hamadryas baboons in very dry, desert-like environments of Ethiopia revealed a social structure based on one-male, polygamous families. However, these portrayals were dismissed as deviations from the typical pattern of baboons which lived in the savannah where early hominids had once roamed: ‘Because he [‘man’] is a gregarious, terrestrial primate with a history of savanna living, it will help us to look at other terrestrial primates with a similar history’ (Tiger and Fox 1971: 28; emphasis added; cf. Fox 1968: 52; Tiger 1969: 46). Baboons were now defended on the basis of their savannah habitat so that the notion of male dominance could be withheld.

The early sociobiology of Tiger and Fox received ample attention from sociologists, biologists and anthropologists alike, just like the popularizations by Ardrey and Morris attracted an enormous lay audience.²⁵ Knowledge of baboons was disseminated to still wider audiences. For a while, ‘the proper study of mankind’ was not man but baboon. The aim was explaining the present human condition, not extrapolating towards the past (Blurton-Jones 1975). Just like rhesus monkeys served as stand-ins for human physiology, savannah baboons served as simplified mechanisms of human behaviour. In cities, in politics, in football stadiums and in bed, modern humans acted with a behavioral repertoire that was quintessentially an adaptation to savannah life two million years ago.

It is not easy to explain the popularity of these grim parallels. ‘Oddly enough,’ a *Playboy* journalist wrote, ‘we seem to be fascinated by and receptive to this depressing news about ourselves. [...] Not only have we accepted that we are the worst of beasts, we enjoy seeing it presumably verified’ (Hunt 1970: 20-1). Without being able to discuss this to the full, the success of such popular science must be related to the important changes in popular culture which took place in the late-1960s, including the rise of occidental misanthropy, the scepticism about

25 The term ‘sociobiology’ is an anachronism in this context but it is generally accepted that the work of Tiger and Fox, amongst others, paved the way for Wilson’s sociobiological synthesis (1975).

Western culture and its relentless exploitation of scarce resources, the enthusiasm for popular science as an alternative form of religion, the emergence of an increasingly secularized generation of young educated people with sufficient spending power to buy popular-science books and sufficient leisure time to actually read them, the possibility to read about sexuality as a scientific theme, and the delight to find a diagnosis of the cultural disease in the biological reality of the human body. It was also in this context that the !Kung Bushmen were hailed as exemplars of authentic humanity, relics of a universal lifestyle based on social egalitarianism, balanced economics, and primeval affluence. The baboons explained where Western culture had gone wrong, the !Kung what it should have been.

More than summaries

With the increase in field studies, fossils and—most importantly—undergraduate students, textbooks did not only become necessary but also lucrative publications in the early seventies. It is here that we first encounter the canonical version of the baboon model as we know it today. Indeed, it is here that we even first encounter the term ‘model’ when talking about the relevance of nonhuman primates in human evolution research. Baboons did not only shed light on present people but also on prehistoric people. Birdsell’s introduction to physical anthropology stated that the baboons, ‘aggressive by nature’, were ‘suitable models for the earliest of ground-living hominids’ (Birdsell 1972: 204). Undergraduates in prehistory and palaeoanthropology could read in Pfeiffer’s *Emergence of Man* (1972: 233): ‘Something of the past may be deduced from the behavior of primates currently at large in grassy savannas—African baboons in particular [who] provide a dramatic example of adaptation to an exposed environment.’ Whereas in *Human Evolution*, Campbell (1967: 339) asserted that ‘the hamadryas baboon might suggest a possible model for [*Australopithecus*] evolving structure.’ From these three introductory books, the baboon model favoured by Campbell was the most unusual because of its reliance on hamadryas baboons. These monkeys, which dwell in the very dry Ethiopian upland, were known through the studies of the Swiss ethologist Hans Kummer (1968). Campbell assumed that as an adaptation to the extreme terrestrial conditions in dry open plains, early hominids, like hamadryas baboons, had developed a structure of one-male units instead of male bonding (1967: 280). The absence of great sexual dimorphism in *Australopithecus* would then be an expression of the reduced male dominance hierarchy. ‘But the analogy between hamadryas and *Australopithecus* must not be pressed,’ Campbell (1967: 339) warned, ‘the two genera are very different, and their behavior and society may be quite distinct.’ Hamadryas or not, the basis for Campbell’s argument was still an ecological analogy and in this he did not differ from Birdsell and Pfeiffer.

Yet Birdsell and Pfeiffer did not only look at baboon studies, but also at the first publications on wild chimpanzees as they dripped in from the field stations where Goodall, the Reynolds and Kortlandt worked. If baboons could tell something about the savannah life of the australopithecines, then chimpanzees, so they argued, represented the previous stage when man’s ancestors had not yet left the trees. It is not difficult to see Jane Goodall’s work in the description Pfeiffer

(1972: 259-60) gives of ‘primates resembling chimpanzees 20 to 25 millions years ago’ who were ‘lively, easygoing, relatively independent creatures, with social systems sufficiently flexible to allow considerable freedom of movement and individual action. Like present-day chimpanzees, most of these forest dwellers moved in and out of shadows and along old ancestral trails.’ Baboons, on the other hand, provided ‘a model of primate life in the grassy savannas of Africa, the sort of world our ancestors encountered when they moved out of open woodlands’ (250). Birdsell concurred with this dual vision: chimpanzees provided ‘a model for dryopithecine behavior’ (1972: 210), whereas baboons were ‘suitable models for the earliest ground-living hominids’ (204). No matter that chimpanzees behaved ‘in the most human fashion’ (Birdsell 1972: 210), they were favourite models for *earlier* stages because they lived in a wooded environment. If today it seems hard to imagine that chimps were once considered to represent an older phase of human evolution than baboons, it only goes to illustrate the pervasion of the argument by ecological analogy in the early seventies.

The function of these early models was mostly didactic. After defending the choice of a particular species, the textbooks continued with a description of the social life of that living species without making any precise inferences about the past. Baboons and chimpanzees served in first instance to give a live image of our most remote ancestors, similar to the Fuegians and Tasmanians in Lubbock’s book a century earlier.

Textbooks are more than summaries of scientific knowledge; through abstraction, simplification and rewriting of the disciplinary history, they channel previous theories into the next generation and steer debates into particular directions. Textbooks do not only resume past knowledge but influence future research; rewriting the history of a debate is an integral part of scientific polemic and rhetoric (Fontijn and Van Reybrouck 1999). The baboon model, in its canonical form, was more an invention of 1970s introductory books than it was a summary of existing theories on the baboon. It was only in these textbooks that the savannah analogy (instead of DeVore’s terrestrial analogy) was fully drawn; it was in textbooks that the word ‘model’ was first used; it was in textbooks that the observed, ecological similarity justified the use of analogies drawn from baboons, hamadryas baboons and chimpanzees. DeVore and Washburn’s baboon model, the contingent, cobbled-together outcrop of their divergent research interests, was now detached from its originators and had been reified into scientific orthodoxy. By thus promulgating the baboon model, textbooks made it respectable and vulnerable at the same time. Alternative models would primarily react against this textbook and popular-science version than against the publications of the original investigators.

Why baboons?

Why was the baboon the first primate source of inspiration for students of human evolution? At its start, serendipity certainly played its part in 1955 when Washburn happened to stay in the Victoria Falls hotel next to a troop of baboons which were incidentally ‘the tamest in Africa’ (Washburn 1983: 16). Yet it would be hard to attribute the whole rise and content of the baboon model to this coincidence alone. The relative

ease with which baboons could be observed also played a role in choosing this animal for fieldwork. Washburn, who had participated in the Asiatic Primate Expedition as a graduate student in 1937, knew the difficulties Carpenter had met when observing the elusive gibbons who swung high in the canopy where foliage was seasonally very dense. As baboons lived on the ground in open environments, they allowed closer and more permanent observations to be made. This ease of access resulted in the tremendous expansion of baboon research during the early sixties (cf. figure 16). However, international postwar politics were also involved: the distribution of baboon species from Ethiopia to South-Africa roughly correlated with the extent of British colonies in East-Africa. If you were an English zoologist or an American anthropologist during the early sixties with an interest in primate behaviour, East-Africa would have been the place to go to. There you found English-speaking colleagues like Louis Leakey or Desmond Clark, there you had Pan-African congresses, there you had baboons.²⁶

By the mid-sixties, baboons (and their close relatives the macaques) were by far the best studied nonhuman primate in the world; many scholars (Washburn being one of them) supposed that their behaviour was exemplary of other primates. This assumption, erroneous as it proved to be, was understandable since the variability of primate behaviour was poorly understood. An interest in ecology, and more precisely in how baboon behaviour was an adaptive response to the environmental constraints of a terrestrial habitat with exposed landscapes, patchy food and relatively high predator pressure, emerged only with a number of younger scholars, particularly through the work of DeVore and Hall. From the mid-sixties onwards, however, baboons were no longer invoked in evolutionary speculations as archetypical monkeys but as terrestrial specialists who had evolved a rigid social hierarchy in response to their demanding habitat. DeVore's and Hall's view of baboon society as an army-like organism with prescribed functions for each subgroup was influenced by both an observational bias and a theoretical stance. Firstly, observation limits intuitively called for such an interpretation of behaviour in terms of age groups and sex classes. Thelma Rowell, a British student of forest baboons, reflects very openly on baboon research, including her own, in the 1960s (in her foreword to Ransom 1981: 9):

Looking at monkeys in the bush it is relatively easy to distinguish between adult males and females and between juveniles of different ages. Adult female baboons especially show easily-read signs of their precise reproductive condition. On the other hand, it is not easy to learn to recognise individuals, beyond a few battle-scarred adult males with absolute certainty. So the early accounts that we wrote described behavior in terms of the interactions between classes of animals, or between characteristic behaviors of classes of animals, in terms of their age and sex.

26 The role of the Pan-African Congress on Prehistory in giving a sense of community cannot be overestimated: the lists of delegates reads like a Who is Who of all those working on prehistory, geology, zoology, and anthropology in colonial Africa (Clark and Cole 1957).

The image of the hierarchically organized baboon society resulted in great part from what was readily visible in a troop. Secondly, the impact of structural-functional anthropologists cannot be neglected (Gilmore 1981; Sperling 1991). Malinowski and particularly Radcliffe-Brown had stipulated a systemic view on human society that consisted of neatly arranged components where individuals took on roles that were controlled by norms and rules. This theoretical framework provided the intellectual background for much early anthropological primatology. Baboon society was seen as an arrangement of constituent parts, each with a specific function, where all aspects of social behaviour were interpreted in terms of function and survival. Washburn repeatedly confessed his admiration for Malinowski and Radcliffe-Brown (1977; 1983; DeVore 1992).²⁷ In an interview with Donna Haraway (1989: 219), DeVore who was trained as an anthropologist recalls the mission Washburn gave him: ‘My marching orders were very straightforward. “DeVore, you’ve absorbed Murdock, Radcliffe-Brown, and Malinowski. Go out and tell us what it’s like with the baboons.”’

If baboons were ever appreciated because they responded to Western notions of male dominance, aggression and power, it must be in the works by Ardrey, Tiger and Fox. The obsession with male bonding, territoriality, and power lay at the base of their publications and baboons revealed the darkest side of humans during the late sixties. Gartlan (1968) has made the interesting point that the key notions of aggression and dominance were survivals of prewar studies on captive monkeys kept in poor conditions where stress was severe and dominance behaviour more frequent. Only a couple of years later, textbooks on human evolution staged the baboon as a living *Australopithecus*, on the basis of a savannah parallel. They often dismissed alternative and less dramatic forms of baboon society because they occurred in forests or deserts.

Sheer contingency, observational access, amount of research, assumed representativeness, ecological analogy, cultural expectation, these were some of the motivations for looking at baboons from an evolutionary perspective. Observational bias and structural-functional anthropologists favoured interpretations in terms of age and sex classes, resulting in the hierarchical, male-dominated, aggressive view of primate society. Though this image was not intended at the outset, it became scientific orthodoxy for about a decade. Indeed, when Washburn’s eye fell on the baboons next to his hotel room in Livingstone during the third Pan-African

27 Washburn had come under the influence of Radcliffe-Brown in Chicago (DeVore 1992: 418); he called Malinowski’s *Argonauts of the Western Pacific* one of the three most influential books he read (Washburn 1983: 6) and saw the work of Yerkes and Carpenter as innovative as Malinowski’s and Radcliffe-Brown’s had been in anthropology (Washburn 1977: 232). His functional explanations of primate anatomy and behaviour were to him extrapolations of structural-functional anthropologists; he even believed that ‘Malinowski’s functional theory probably works more usefully for monkeys than for human beings’ (Washburn 1983: 17). Yet despite this nominal allegiance to Malinowski and Radcliffe-Brown, ‘the notions of function and social system in Washburn’s physical anthropology owed more to comparative evolutionary biology than to the analyses of either of these social theorists’ (Haraway 1989: 205). The functionalist impact might have been more nominal than substantial in Washburn’s case, but this cannot be said about the work of someone like DeVore. Indeed, the functionalist ideas of Radcliffe-Brown ‘were quite congruent with the then-ascendant evolutionary view that individual animals acted for the good of their society (and ultimately, for their species), rather than out of more selfish reasons’ (Gilmore 1981: 391).

congress, there was no way he could predict that the ten-year old son of the conference co-organizer would forty years later, together with his ghost-writer, express the most extreme version of the baboon model:

I have always thought it reasonable to imagine early hominid social life as analogous, in some strictly circumscribed ways, to the social life of savannah baboons. Baboons live in troops, some small, some compromising as many as a hundred individuals. [...] [Australopithecine] habitat would have been similar: patchy, open woodland and some gallery forest, offering a range of plant foods: nuts, fruits, shoots. No doubt they foraged for grubs and birds' eggs, just as baboons do today. No doubt, too, they occasionally captured young antelope and the young of monkeys, just as baboons do today. [...]

In this sketch the principal difference between these hominids' behavior and what we see in modern savannah baboons is the hominids' mode of locomotion: bipedal as against quadrupedal. Everything else is imagined to be similar to any large primate that forages in relatively open country for a largely vegetarian diet.

(R. Leakey and Lewin 1992: 137-9)

Social carnivores and geladas

What do Ethiopian gelada baboons, Alaskan wolves and Tanzanian hyenas have in common? Very little, apart from the fact that they all seem rather unlikely candidates as models for human evolution. And yet, this is precisely how they were seen in the early 1970s after the baboon model had become textbook knowledge and before the chimpanzee model gained its full momentum. Following an old hint by Washburn, social carnivores (wolves, hyenas, wild dogs and lions) were thought to resemble early hominids in their hunting behaviour. This reliance on hunting, particularly on big game, required the coordination of individual efforts to the extent that it largely dictated the social structure of the species. The causal train ran from subsistence to society. The few advocates of the gelada model, however, took one step down by looking at dentitions to deduce diets. Gelada baboons constitute a different genus among the cercopithecines; they live in the rocky Ethiopian upland, have bright-red triangular skin patches on their breast, shuffle on their haunches, and live on grasses. In many respects, gelada teeth were like the australopithecine dental features, so that the gelada subsistence base of small-object feeding might have been similar to what australopithecines ate. Gelada modellers looked into mouths to learn about menus; carnivore modellers looked at menus to learn about mores.

Despite this difference in theme, both relied on a rather distant source to predict hypotheses on early hominids. None of them denied the many differences between source and subject, yet they believed that some structural similarities might be more relevant than multiple formal resemblances. The purpose was not so much a wholesale projection of living animals to fossil bones, but a selective transfer of attributes from one side of the analogy to the other. The discussions surrounding these models were thus far more technical than anything hitherto encountered (this is especially true of the gelada model)—which explains why they

never were really popular. Another reason is that many of the substantive claims eventually turned out to be simply wrong (this is especially true of the carnivore model with its undue reliance on hunting). No matter the many semantic weaknesses, in a syntactic sense they provided the most valid forms of analogy. Not surprisingly, some preferred the term analogy instead of model. Jolly (1970) and Dunbar (1976), who looked at geladas, spoke of their ‘baboon analogy’; King (1976) referred to the ‘carnivore analogy’.

From subsistence to society: the social carnivore analogy²⁸

From Hobbes’ *‘homo homini lupus’* to Spengler’s *‘Der Mensch ist ein Raubtier’*, Western thought has often tried to elucidate human nature by reference to carnivores, a mental exercise commonly founded in misanthropy. The presence of wolves and lions in fairy tales and fables equally testifies to this extra-scientific fascination with predators—just think of Romulus and Remus, Little Red Cap, King Noble, Werewolves and the Lion King (Lopez 1978). It was only in the late 1960s that the importance of these animals for evolutionary purposes started to be explored.²⁹

Man the diurnal hunter

‘The relevance of carnivore behavior to the study of early hominids’ was the title of a paper which appeared in the *Southwestern Journal of Anthropology* in 1969. Its authors, George Schaller and Gordon Lowther, urged to look more closely at carnivores as these offered ‘numerous possibilities towards the elucidation of the origins and form of social organization in man’ (336). To start looking at carnivores was a brave change of plans for someone like Schaller who had spent years observing gorillas (Schaller 1964) and who was the world authority on them before Dian Fossey began her fieldwork. The 1960s increase in field studies, however, had laid bare the enormous variability of primate behaviour (even within the same species), so that every attempt at defining a prototypical primate pattern was rapidly frustrated. Following the ecological paradigm, Schaller and Lowther saw modern primate behaviour as a function of its environment, not as an ancestral relic. ‘Since social systems are strongly influenced by ecological conditions,’ they wrote (1969: 307), ‘it seemed that it might be more productive to compare hominids with animals which are ecologically but not necessarily phylogenetically similar, such as the social carnivores.’ Ecological similarity did not just refer to a similar environment (otherwise baboons would have still been appropriate) but to a similar way of exploiting that environment, that is by hunting. Ecological analogy meant here ‘subsistence analogy’. For Washburn and DeVore, early hominids, like baboons, had had to defend themselves against predators; for Schaller and Lowther, early hominids *were* the predators. Some of the difference in emphasis

28 Strictly speaking, these are of course not primate models but since they made a considerable impact on the field of palaeoanthropology and primatology, they need to be taken into account.

29 An early example is Carveth Read’s vision of our ancestors as wolf-apes, ‘*Lycopithecus*’, ‘who co-operated in hunting’ and lived in ‘a society entirely different from that of any of the Primates [...] and most like that of the dogs and wolves—a hunting pack’ (1920: 39; cf. Cartmill 1993).

can be felt in the choice of words. DeVore and Washburn (1963: 335) spoke of 'the problems which confronted early man', whereas Schaller and Lowther (1969: 336) investigated, more optimistically, 'the broad spectrum of behavioural possibilities that were open to early hominids'.

The substantial part of their paper, however, did not greatly differ from the previous work on baboons in that it was equally rather descriptive than argumentative. Schaller and Lowther summarized field studies on lions, leopard, cheetahs, spotted hyenas, wild dogs and jackals with regard to group dynamics, dominance, leadership, territoriality, and so on, while also integrating evidence from primate studies and hunter-gatherer ethnography. If a trait, like male dominance, was found in all three classes (humans, primates, carnivores), it was directly attributed to the early hominids as well. If a trait was confined to the carnivores alone, it could still be projected into the past. This was the case for inter-specific aggression, cooperation and food-sharing. The basis for these inferences was, of course, hunting. In logical terms: if hunting was the observed similarity between carnivores and early hominids, cooperation, food-sharing and aggression were predicted similarities for the target analogue.

But was hunting really an *observed* similarity? Of course it was not, at least not on the target side of the analogy. No one had seen an australopithecine making a kill, yet the hunting early hominid had become an icon of palaeoanthropology ever since Dart's misanthropic visions, Leakey's spectacular discoveries, Isaac's early analyses and, of course, the Man the Hunter conference in Chicago, 1966. As mentioned above, the resulting book of that conference (Lee and DeVore 1968) presented not so much the inauguration as the epitome of the hunting hypothesis. It asserted that humans had been hunting for over one million years (99% of the human career) and that hunting was 'the master behavior pattern' (Laughlin 1968: 304) responsible for all major changes in human evolution. However, the hunting hypothesis rested on rather shaky empirical foundations, its iconic exemplars from the present world being the overwhelmingly herbivorous baboons and the predominantly vegetarian Bushmen. Primates could simply no longer do. The carnivore model, not the baboon model, was a direct reflection of this conviction.

Schaller and Lowther attempted to demonstrate the viability of hominid hunting by some small experiments in the Serengeti. They consisted of hiking and seeing how much meat could be collected by either picking up young, crouching animals (like Thomson's gazelle fawns), scavenging, or by chasing sick, old or isolated animals. The following quote gives an impression of the charming enthusiasm of these pioneer days:

In the course of our wandering we also came across a sick, abandoned zebra foal, weighing about 40 kg. We captured it after a brief chase (and then released it). A young giraffe, weighing some 150 kg., behaved abnormally; after stalking to within a few meters from it, we found that it was blind. One of us grabbed its tail to simulate capture. (328)

Zebra foals, blind giraffes, and tail-grabbing, Schaller and Lowther concluded that ‘under similar circumstances a carnivorous hominid group could have survived’ (328).

There was, however, one noticeable difference with carnivore hunting: hominids were believed to hunt during daytime, carnivores at night. What happens here is something curious in the logic of analogy: whereas dissimilarities are often believed to weaken the argument, this one *reinforced* it. Because by being nocturnal predators, the social carnivores left an ecological opening for a social predator hunting large animals during the day. Nocturnal and diurnal hunting habits were complementary rather than conflicting. As said previously, a certain distance between the source and subject analogue is often more productive than at first sight seems. It was this lack of fear for a distant analogue which had led Schaller and Lowther to look at social carnivores in the first place.

Strengthening a distant analogy

In the wake of Schaller and Lowther a number of authors further explored the possibilities of a carnivore analogy (Cachel 1975; King 1975; 1976; 1980; Peters and Mech 1975; Peters 1978; Thompson 1975; 1976; Hall and Sharp 1978). They all agreed that phylogenetic closeness was not determinant: ‘While it is true that African apes are the closest living relatives of man,’ Susan Cachel (1975:194) wrote, ‘this does not necessarily make them the logical choice for an evolutionary model.’ Hall and Sharp (1978: 3) coined the aphorism that ‘anatomy follows behavior’, not vice versa. Studying behavioural correlates (social hunters) was therefore more important than looking at phylogenetic and anatomical neighbours. Rather than discussing all proposals separately, I prefer to focus on how they defended the choice of their (more distant) source. Which criteria did they invoke to strengthen their analogies based on carnivores?

A first and simple answer was that including carnivores in the analogy enlarged the number and variety of source contexts used. This was Glenn King’s strategy (King 1975; 1976; 1980; cf. Peters and Mech 1975). Not confining himself to studying either nonhuman primates or hunter-gatherers or social carnivores, he tried ‘to coordinate these rich sources of inference about early hominid behavior’ (King 1980: 107). Thus in his studies on socioterritorial units, he found that a fission-fusion system (large stable groups which divide into smaller subgroups) was not restricted to carnivores only but could also be seen in some nonhuman primate species who occasionally practised hunting (like chimpanzees) and among hunter-gatherers. The three lines of evidence were ‘in complete harmony’ (King 1976: 330). He saw it as a form of convergent evolution, an adaptation to hunting, and postulated a similar social structure for early hominids. Carnivores strengthened the analogy because they enlarged the source sample. Since he worked on species as diverse as hyena, wolf and lion, the variety of the source was also increased.

Others, however, believed that carnivore data was not just complementary but far superior to observations on primates. According to Thompson (1975: 113) there was ‘uncontrovertible evidence of the convergence of human behavior with carnivore behavior’. This was defended by summoning the number of similarities

between the two sides of the analogy, a second criterion. In their book on wolves (with the promising title *Wolf and Man: Evolution in Parallel*), the editors called the similarities between both wolves and humans ‘impressive’ (Hall and Scharp 1978: 2):

Both are social animals whose living units are relatively small numerically. Both are intelligent, clearly more intelligent than the prey on which they subsist. Both are capable of extreme physical exertion and, more important, of sustained physical exertion at a relatively high level. Both exploit open ground and broken forest areas. Neither species has any single physical attribute that allows it either to overpower or to outrun large prey at will. The two species are about the same body weight and, though not among the largest animals in the world, are virtually immune from predation.

Some of these similarities are obviously more important than the others. That wolves and humans are about the same body weight is not relevant (and, in fact, not even true). There were as many differences as similarities. Washburn, sticking to his primate models (and thus keeping silent about his early hints at a carnivore model), did not hesitate to stress the immense differences between carnivores and humans. The carnivore analogy, he wrote together with Moore, ‘completely misses the special nature of the human adaptation’ (Washburn and Moore 1974: 135):

Human females do not go out to hunt and then regurgitate to their young when they return. Human young do not stay in dens, but are carried by their mothers. Male wolves do not kill with tools, butcher, and share with females who have been gathering. A human mother who hunted like a wolf or wild dog would have to run a two-minute mile carrying a baby.

King (1980: 101) quoted these lines and replied to them:

These statements are correct, but they do not constitute a general argument. While there are certainly differences between humans and carnivores, there are also differences between human and nonhuman primates, which Washburn and Moore advocate as a source of inferences about early hominids. [...] Male chimpanzees do not kill and butcher with tools, nor do they exchange meat for plant foods gathered by females. [...] The fact that one carries the meat in his hands and the other in his stomach should not obscure the socioecological analogy.

Washburn and Moore indicated dissimilarities between humans and wolves, King replied by showing dissimilarities between humans and chimps. What is really at stake in this amusing tug-of-war is the logical question as to what counts as relevant similarity (and dissimilarity) and what not. More than anything else, this debate shows that (dis)similarities can never be discussed per se. They gain their meaning only insofar they are relevant, i.e. causally connected to what we want to infer. This brings us to our third strengthening device.

Many of the carnivore advocates attempted to indicate a causal connection between different aspects of the source analogue. King, for example, argued that among carnivores the existence of a flexible social system of large, stable groups with fluid subgroups was caused by ‘the selective pressures connected with the advent of hunting’ (King 1976: 329), implying that if hunting was present with early hominids, a similar social system would have arisen. Peters (1978) showed that big-game hunting over large areas among wolves required the existence of cognitive maps and communication, thus suggesting that early hominids had similar devices (he even guessed that cognitive maps might have been at the base of human language). The most original argument, however, comes from a paper by Thompson (1976). Written in a rather abstruse style, it can be summarized as follows:

1. Gracile australopithecines, following Dart’s study of the Makapansgat site, exhibit ‘the behaviors of high intraspecific killing, cannibalism, and attacking larger carnivores’ (Thompson 1976: 554).
2. Of all living carnivores and nonhuman primates, only three species show the same set of behavioural patterns: the wolf (*Canis lupus*), the hyena (*Crocuta crocuta*) and the lion (*Panthera leo*).
3. These three species share another number of behaviours, which do not occur together in any other carnivore or nonhuman primate species: territoriality, multi-male groups, carnivorous diet (over 50 % meat).
4. Causal relations between these two sets exist. For example: since high intraspecific killing is only possible in species where enough males are around to ensure group survival after some of its members have been killed, intraspecific killing is causally related to multi-male groups. Correlations for the other aspects can also be found.
5. Therefore, gracile australopithecines ‘probably had a diet comparable to the modern carnivores and lived in resident, multi-male groups’ (Thompson 1976: 554).

The interesting thing about this argument is not so much its conclusion but its form: it is a classic analogy which moves from an observed similarity (step 1 and 2) to a predicted similarity by a transfer of attributes present in the source (step 3) to the target (step 5), following some causal connections in the source (step 4)—or ‘behavioral correlations in living species’ as Thompson (1976: 552) called them. The weakness of the argument lies, undoubtedly, in its first premise. Dart’s ideas were highly controversial and it is unforgivable that Thompson, writing in the mid-70s, still relied on Dart’s work from the 1940s and 50s while so much other studies had been done in the meantime. (Another weakness is that his causal connections are not always as neat as the one above; his explanation of cannibalism, for example, is rather contrived.)

Enhancing the number and variety of source contexts, adding similarities and establishing causal links within the source were all strategies used by those who preferred a carnivore analogy over a primate model. Hall and Sharp (1978: 4) were very clear about it: ‘Though the great apes [...] are relatives of man, they are relatives who have adapted in a different direction.’ Through the reliance on

hunting, hominids and carnivores, no matter how unrelated, displayed ‘parallel adaptations’ (King 1975: 69), ‘convergence’ (Thompson 1975: 113) or ‘evolution in parallel’ (Hall and Sharp 1978).

Hunting under fire

Hunting was the pivotal point around which the whole carnivore analogy turned. As long as the conference conclusions of Man the Hunter, Isaac’s interpretation of living floors or even Dart’s hunting hypothesis were sustained, carnivores could be successfully invoked. Some authors even started their articles by explicitly assuming (and accepting) the importance of hunting: ‘This view assumes that hunting was an important part of hominid ecology’, Peters (1978: 95) wrote, while King (1975: 69) required ‘the assumption that hunting was a major factor in human evolution’ (King 1975: 69). As a consequence, the observed similarity between source and target analogue could never be more than an *assumed* similarity: hunting was not a material, Binfordian static that could be observed in the archaeological record but an interpretation of that record.

The ease with which hunting was accepted came under fire by the late 1970s (cf. Harding and Teleki 1981). Attacks came from different angles, including primatology (Teleki 1975), feminist anthropology (Tanner and Zihlman 1976; Zihlman 1978; Tanner 1981), hunter-gatherer ethnography (Hayden 1981) and taphonomic archaeology (Brain 1981; Isaac 1984; Binford 1981; 1988a). Primatologists indicated the amount of hunting among nonhuman primate species, ethnographers documented the importance of foraging among hunter-gatherers, feminists decried the androcentric bias of Man the Hunter, and archaeologists criticized the speculative reasoning and the poorly understood archaeological record.

Around 1980 it became clear that early hominid hunting was a much more contentious issue than had been previously estimated. This doubt sealed the fate of the popularity of social carnivore analogies. In 1978, Hall and Sharp published their book on ‘the wolf as a model for human evolution’ (6) believing they were on the threshold of a new research programme ‘comparable to the primate literature at the time DeVore and Washburn (1961) articulated their dominance model of baboon social organization’ (207). In fact, they were at the end of what had always been a fairly minor stream of thought compared to the baboon and chimp business. Perhaps this explains why their book is so very rarely quoted. It turned into oblivion, and got lost on the bookshelves of natural history museums rather than of anthropology departments where it was destined for.

From dentition to diet: the gelada analogy

Was hunting the basic assumption of the social carnivore analogy, proponents of a gelada model were far less assertive when it came to establishing the nature of early hominid food resources. In fact, for them diet was not the premise but the very problem. Although I found only three articles explicitly dealing with such

analogy (Jolly 1970; Szalay 1975; Dunbar 1976) and even if their popularity was rather limited, they are worth looking at in some detail as they all present interesting forms of reasoning.

The seed-eaters

The first and most influential proposal was Clifford Jolly's 1970 article in *Man*, entitled 'The seed-eaters: a new model of hominid differentiation based on a baboon analogy' (1970).³⁰ Even if the ultimate aim of the paper was to come to 'a convincing causal model of hominid origins', large part of it was dedicated to a discussion of an analogy based on *Theropithecus*, the genus of which the gelada is the only living species today (Jablonski 1993). Jolly took issue with the accepted orthodoxy that tool use was the first major step in human evolution, intensely correlated with upright bipedalism and canine reduction (Washburn and Avis 1958; Washburn and DeVore 1961).³¹ Discontent with such 'artefactual determinism' (6), he argued that if toolmaking was of any importance in changing hominid teeth, signs of it should 'appear in the fossil record at least as early as the first signs of dental reduction, rather than twelve million years later' (7). He equally questioned 'the current obsession with hunting and carnivorousness' (8-9) which was undoubtedly corroborated by observations of chimpanzee hunting. But if chimpanzees hunt and if hunting is a truly important evolutionary factor, why then did chimpanzees not evolve into humans? Jolly rightly said that 'it is illogical to invoke the behaviour of living apes to explain the origin of something that they themselves have not developed' (9). This fundamental critique could be brought up against most of the chimpanzee models, as the next section will show.

If tool use and hunting could no longer explain the earliest phase of human evolution in general, and the remarkable canine reduction in particular, what else could? Seed-eating, Jolly answered. Rather than devouring meat, grinding cereals was what made us human. Seed-eating was consistent with a life in an open-country habitat, with the general, postcranial anatomy of 'basal hominids' (Jolly's term) and with the particulars of hominid dentition. He arrived at this original conclusion through an analogy with the genus *Theropithecus*.

The choice of Jolly's source was not prompted by some immediate resemblances with living great apes. Instead, he wrote: 'We must look *outside* the normal behaviour of apes for a factor which agrees functionally with the known attributes of early hominids' (9, original italics). In this sense, hominids were as much outsiders as *Theropithecus*:

30 The term baboon analogy might cause some confusion. Jolly does not side himself with the traditional baboon models but uses the term baboon *sensu latu*: apart from *Papio*, it also includes the genus *Mandrillus* and *Theropithecus* to which the gelada (*Theropithecus gelada*) belongs.

31 Wiktor Stoczkowski (1994: 96-106) has convincingly demonstrated the tenacity of simplistic causal explanations of the sort: bipedalism implies free hands, free hands imply tool-use, tool-use implies canine reduction, etc. This line of reasoning states that tools replaced teeth both in food acquisition and preparation as well as in defence. With a wink to Leslie White, it can be said that choppers were 'man's extrasomatic teeth'.

*many of the characters distinctive of basal hominids, as opposed to pongids, also distinguish the grassland baboon *Theropithecus* from its woodland-savannah and forest relatives *Papio* and *Mandrillus*, and are functionally correlated with different, but no less vegetarian, dietary habits.* (9)

Both hominids and *Theropithecus* had deviated from their ancestral stock by moving into open grasslands. But there were further similarities. Jolly listed the adaptive characters distinguishing hominids from pongids and the genus *Theropithecus* from *Papio* and *Mandrillus*. He found that the postcranial, cranial and dental adaptations of both hominids and *Theropithecus* showed several convergences: both had flexible hands attuned to a precision grip; both had well-developed mastoid processes, correlated to bipedalism or truncal erectness (modern geladas mainly sit on their buttocks while feeding, requiring strong muscles to keep their heads upright)³²; both had their temporal muscles adapted to heavy chewing in the molar region of the mouth rather than tearing, stripping, and nibbling in the incisal region. Most importantly, both had narrower and smaller incisors and bigger molars compared to their woodland and forest relatives. So far for the observed similarities (Jolly lists some more, but these are the essential ones), what about the causal relations in the source?

Jolly noted that *Theropithecus* ‘eats small food objects requiring little incisal preparation, but prolonged chewing’, while *Papio* ‘concentrates on fleshy fruits and other tree products, most of which require peeling or nibbling with the incisors’ (14). Theropithecine anatomy was by and large an adaptation to this form of small-object feeding: the versatile hands were necessary for collecting food-items like grass-blades, seeds and rhizomes; truncal erectness (sitting upright) allowed rapid gathering with two hands of small food-objects; greater muscular power in the molar region enabled heavy chewing of harder and more fibrous food items; big molars enlarged the grinding surfaces; small incisors permitted better molar occlusion required for grinding. Canine reduction, in this view, was not an independent process but ‘a secondary effect of dietary influences on incisors and molars’ (16): the canines became smaller because their neighbouring incisors had become smaller.³³

As a consequence of the observed similarities between geladas and hominids and the causal connection in the source, a further similarity could be predicted. Jolly stated that there was good reason for ‘attributing the *Theropithecus*-like incisal proportions and jaw characters of the early hominids to a similar adaptation to a diet of small, tough objects’ (14). If *Theropithecus* was a small-object feeder, so were early hominids. Here was a case of ‘evolutionary parallelism’ (12).

Yet there were also a number of differences between the source and target analogues. Jolly acknowledged that most parts of the hominid postcranial skeleton, with its adaptation to upright bipedalism, were distinct from *Theropithecus*,

32 As a consequence of an upright trunk, Jolly argues, epigamic features of both hominids and *Theropithecus* changed. The cape of fur on the shoulders of adult male geladas and human facial hair would be instances of parallel evolution. Idem for the ventral and pectoral position of the female epigamic features in both taxa.

33 This might be true, but Jolly somehow obliterates the fact that extant geladas still have the longest canines among primates (Fedigan 1982: 316).

though this did not affect the analogy (as it was not causally linked to diet). Other differences were more relevant and thus required more attention. For example, *Theropithecus* molars showed considerable crown complexity resulting in a ‘bumpy’ surface (a phenomenon known as hypsodonty, which is frequent among grass-eaters like kangaroos); hominids, instead, had more even cusps with thick enamel wearing to flat surfaces. On top of that, the anatomy of the hominid jaw also allowed rotary chewing rather than mere mincing as with *Theropithecus*. And the parabolic shape of the hominid dental arcade differed from the V-shaped theropithecine mandible. All these differences led Jolly to assume that hominids had had a slightly different diet. Whereas *Theropithecus* is a specialized grass-eater, hominids must have subsisted on ‘small, hard, solid objects of more or less spherical shape’ (17). Seeds were the only staple food which fit that description. Rotary chewing and flat molar surfaces enhanced the breakage of these seeds, whereas a parabolic dentition gave more space to the tongue in mastication. As such, the observed differences could be accounted for by a slightly different subsistence base. Instead of grass, Jolly concluded that ‘the diet of basal hominids was probably centred upon cereal grains’ (18).

From this finding, he went on to erect a two-staged model of human evolution consisting of a seed-eating phase followed by a later phase in which meat took on a greater role. Robust australopithecines, in this view, would have remained in the first phase—an idea in line with the nickname the Leakeys had given to the type fossil of *Australopithecus boisei*: ‘nut-cracking man’. Between a reliance on fruit among our dryopithecine ancestors and our reliance on meat as full-fledged members of *Homo*, Jolly inserted several million years of ‘muesli-eating’ in the australopithecine phase. The earliest step in human evolution did not involve a transition to meat-eating but ‘a shift from one kind of vegetarianism to another’ (Jolly 1972: 9).

If I have dwelt rather long on Clifford Jolly’s argument, it is because he presents us with a fascinating example of analogical reasoning based on observing similarity, establishing causality, and accounting for differences. Yet it is precisely on his appreciation of difference that two later critics would aim their arrows.

Meat, fruit and roots

A dental expert, Frederick Szalay (1975) replied to Jolly’s paper in the same journal *Man*. Cast in a rather technical and offensive prose, he argued that while Jolly was right in deducing a graminivorous diet from the dentition of *Theropithecus*, the extrapolation of this inference to the hominid realm was far less certain. Firstly, because Jolly’s characterization of the hominid complex showed a bias towards the more robust australopithecines; secondly, because even if teeth of both taxa probably presented similar evolutionary tendencies (smaller incisors, larger molars), the actual form of these teeth remained too distinct to be lumped: the gelada’s hypsodonty was quite unlike the even, enamel-covered molars seen in hominids. There was a ‘lack of parallel between the baboon-protogelada and the pongid-protohominid transitions’:

Indeed, divergent morphologies and functions characterise the two morphotype dentitions. The conclusion is inescapable, therefore, that the biological roles performed by the divergent morphologies and mechanical functions were equally dissimilar. (425)

Human canines had not been reduced simply as a corollary of smaller incisors; rather, their smaller size had been positively selected for. Canine reduction was not about the *loss* of size but about the *gain* of an alternative character state. The advantage of smaller canines is that they can be aligned with the incisors, resulting in a sharp ‘edge-to-edge bite’ (Szalay 1975: 428): ‘Strong vertically implanted incisors plus incisiform canines become the tools which grasp and tear meat, tendon, and fascia’ (428). So, we are back to meat-eating. The molars with their thick enamel and even surfaces were ‘a great selective premium for either withstanding the abrasive effects of cracking ribs, metapodials, joints, or for increased longevity, or for both’ (428)—actions for which the delicate, gelada-like cusp pattern would have been inappropriate. Not surprisingly, Szalay concluded by reiterating the hunter-scavenger model.

On the basis of the same incisors, canines and molars, Szalay came up with an entirely different conclusion, independent from a particular living primate. To him, the differences between *Theropithecus* and hominid teeth were simply too large. There had not been any observed similarity to start with.

To complicate matters further, Robin Dunbar (1976) came up with yet another vision. Although he believed that there still was a genuine observed similarity between both types of dentition, he wondered whether the causal pattern suggested by Jolly was the only possible one. Unlike Szalay, Dunbar was not a dental expert but had studied gelada behaviour in the wild. He found that geladas were very specialized graminivores who fed on different parts of the grasses throughout the annual cycle (leaves in the wet season, roots in the dry season, and seeds in between). As all these required heavy chewing, gelada dentition was indeed highly adapted to grinding. So were hominid teeth, but through different means: in order to obtain efficient occlusion of the molars necessary for chewing, hominids had lowered the canines (canine reduction), whereas the gelada had raised the molars (by hypsodonty).³⁴ The effect was still the same. Dunbar, unlike Szalay, believed that ‘the differences in dental morphology between *Australopithecus* and *Theropithecus* need not [...] necessarily reflect marked differences in diet’. To him, this difference in molar pattern was not relevant and the observed similarity remained untouched.

Dunbar’s doubts were of another sort. Whereas seed-eating might have been responsible for the sort of dentition typical of early hominids, a diet based on fruits and roots could equally cause a similar pattern:

In either case, the dental adaptations would have been similar. Not only do roots and tubers require considerable mastication prior to ingestion, but most of the fruits to be found in savanna habitats are of the small, “hard” type with little or not soft outer parts, and would lead to heavy wear on the cheek teeth. (165)

³⁴ Following an old line of argument, Dunbar argued that hominid tool use permitted the loss of large canines (cf. note 34).

So, Dunbar wondered, was a cereal subsistence or the ‘fruit-and-root’ diet responsible for the hominid teeth pattern? If cereals, hominids—like the geladas Dunbar observed—could have relied year-round on this single resource. But since we know that hunting *did* evolve in human evolution, and since animal matter *did* play a certain role in australopithecine diet (a notion Dunbar borrows, rather uncritically, from Wolpoff), it cannot have been the permanently available cereals. The fruit-and-root diet, on the other hand, ‘would have imposed conditions of periodic fruit shortage’ which ‘promoted meat-eating as a supplement to a seasonally deficient diet’ (165). Early hominids, following this line of reasoning, must have been small-object feeders, but more on hard fruits and roots than on cereals.

The problem Dunbar’s paper highlights is that of ‘equifinality’, namely that the same effect (in this case dentition) can be caused by different processes (here: two different sorts of diets). He presented an argument to select one of the two processes, but it rested on the rather uncritical assumption that meat was indeed part of the hominid diet.

Both Szalay and Dunbar offered a critique on the original gelada analogy, but they did so from diametrically opposed angles which can be summarized as follows: Szalay’s doubts were on the ‘horizontal’ relation of similarity (are both dentitions really similar?); Dunbar’s doubts on the ‘vertical’ relation of causality (is seed-eating the only cause for the observed dentition?). The resulting conclusions were totally at odds. Whereas for Jolly, early hominids had been eating seeds, for Szalay, their diet had consisted of meat, and for Dunbar, it had consisted of fruits and roots, with a seasonal bit of meat. Deducing diet from dentitions is apparently no easy business, if even experts differ so strongly. For outsiders of this fairly technical discussion, it is sometimes hard to discern which arguments point to genuine evolutionary processes and which are just *ad hoc* solutions (Fedigan 1982: 314–7). Perhaps the technicality of the subject matter and the resulting indecision explain why the gelada analogy was never further pursued, despite its logical consistency. Indeed, it requires quite some devotion to skim through phrases like ‘the short basi-occiput, and anterio-posteriorly narrowed articular fossa’ without getting a conclusive answer! Of the 19 contributions to a recently edited volume on the genus *Theropithecus* (Jablonski 1993; which is perhaps the best synthesis available on any primate genus), not one staged geladas as a model for early hominids.

Remote sources and logical consistency

In a formal respect, the arguments based on social carnivores and geladas were all cases of valid (or rational) analogies, irrespective of their resulting claims. Formal validity, of course, does not guarantee the truth (or plausibility) of the conclusion—since this is equally dependent on the truth of the premises—but it is at least a necessary prerequisite of the analogy.

It cannot be accidental that in the history of postwar models, the greatest logical consistency was achieved with the most remote source analogues. Not seduced by the richness of immediate, but often irrelevant, similarities stemming from our closest living relatives, carnivore and gelada modellers sought to find genuine

resemblances and causal connections. Their models were examples of ‘imported’ instead of ‘manifest analogies’ (Leatherdale 1974). Rather than finding an all-embracing life image of our ancestral past, the gelada analogy served to solve a localized problem—without pretending that early hominids were ‘really like’ them. Similarly, social carnivores were never said to be living australopithecines; in some respects they simply showed a number of convergent adaptations.

Their logical qualities notwithstanding, the carnivore and gelada analogies never made it into widely disseminated, regurgitated and popularized textbook knowledge. For this, the source analogues were perhaps too distant, the intrinsic qualities for imaginative reconstruction too limited, the discussions too technical (in case of the gelada) and its premises, especially hunting in the carnivore model, too much accepted at face-value. The multifarious attack against the hunting hypothesis in the late 1970s—in particular the taphonomic study of the Plio-Pleistocene record, the reassessment of ethnographic and primatological evidence concerning meat-eating, and the feminist-anthropological critique—undermined the credibility of any model built on social carnivores. Ten years after the glorious vocabulary of Man the Hunter, early hominids were believed to be displaying a range of activities, but chasing mammals was less and less one of them. By 1980, the idea that hunting had been the very driving force behind human evolution was soon regarded as one of the sweeping fantasies of the 1960s.

In theory, Jolly’s seed-eating hypothesis could provide a plausible and well-founded alternative now that meat-eating had fallen into disgrace. Yet this never really happened: it just remained ‘Jolly’s seed-eating hypothesis’ as it continued to be referred to even after ten years (Fedigan 1982: 314–7). It was perhaps gratifying for the author that his name kept being mentioned in later renditions of the idea which is still around, but it was detrimental to the idea itself. In a sense, the success of a theory can be read off of the degree of detachment from its original designer. Due to textbook transformations and popular science writing in the early seventies, the baboon model had become disconnected from its originators Washburn and DeVore; it was turned into what one could call ‘public knowledge’. Ironically, the highest praise an author can receive for an idea is that his or her name be forgotten and disconnected from it. Only disconnected ideas live a life of their own. Jolly’s seed-eating hypothesis never reached that stage. At the time hunting became discredited, a more powerful alternative than the grass-nibbling gelada—a species few people had heard of anyway—started to fire the minds of anthropologists: the chimpanzee. And although the earliest claim for a chimpanzee model drew upon Jolly’s work, in all later formulations the geladas were dropped out of it. They returned to what they had always been to most anthropologists: a zoological curio dwelling in the Ethiopian upland, where they shuffled on their buttocks, picked up grasses, and displayed their bright pectoral patches—not the sort of stuff human ancestors were made of.

Wolves, hyenas, lions, geladas—during the first half of the 1970s, their behaviour or diet was interpreted in terms of convergence to the hominids, rather than divergence from them (as with the apes). Analogy was at stake here rather than homology, to use the terms in their traditional biological meaning. The purpose was

not projective modelling, but analogical reasoning. Or to re-use the twin concepts of which the Victorian social evolutionists were so fond of, we are dealing here with cases of ‘independent invention’ rather than ‘historical connexion’.

The chimpanzee model was a different story altogether.

Chimpanzees

It is hard to imagine a primate species which has made a larger impact on human evolution studies than the common chimpanzee. Once an elusive animal in the wild (cf. Nissen 1931), from the 1960s onwards the results of fieldwork on the chimpanzee have not ceased to surprise scientists and non-scientists. Starting with small scale research projects by the British (Goodall at Gombe) and the Japanese (Itani at Mahale) in Tanzania, studies of chimpanzee behaviour have expanded tremendously during the following decades, making it the best studied great ape to date (Hebert and Courtois 1994; figure 17).³⁵ Time and again, chimps seemed to be much more human than was previously thought possible: they used tools, they made tools, they shared food, they showed occasional bipedalism, they hunted, they hunted cooperatively, and so on. This behavioural similarity, coupled with a genetic affinity, made chimps interesting creatures for modelling human evolution.

However, it is nearly forgotten that originally chimpanzees were studied as ecological analogues, not phylogenetic homologues. Louis Leakey did not simply want Goodall to study these animals as such, but in particular to study them in the Gombe region—a reserve whose open woodland vegetation bordering on a lake shore, he believed, was comparable to the environment of the hominid sites he had excavated. His instrumental role in awakening a palaeoanthropological interest in chimpanzees had to do with an assumed ecological analogy, not so much behavioural similarity. The rationale for studying chimps eventually changed (Goodall 1986). The popular image of Gombe increasingly became one of a dense, impenetrable forest, and the occasional activities of the chimps on the beach of Lake Tanganyika received less attention from scientists and popular-science writers, although this was the place where Goodall and her family actually lived (1990: 22-3). This changing representation of the Gombe environment was paralleled by a changing appreciation of chimpanzees’ role as models. Just like baboons had originally been studied because of their observability, and were eventually favoured because of ecological similarity, chimpanzees were initially studied because of ecological analogy, and were only later defended on the basis of phylogenetic homology and behavioural similarity.

However, at the base of the fully developed chimpanzee model laid not so much a fascination with chimps but a frustration with anthropologists, male anthropologists. More than Goodall and her colleagues, the chimp model was forged by a number of North-American feminist anthropologists.

35 At the same Jane Goodall drew world-wide attention with her work, the Reynolds and Kortlandt were equally observing chimpanzees in the wild.

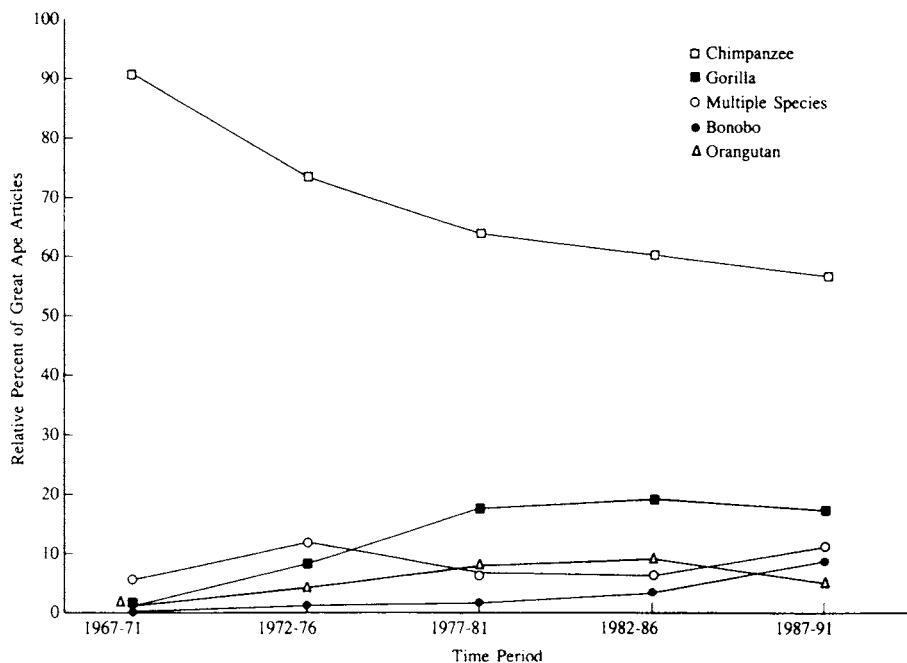


Figure 17. *Chimpanzees are by far the best studied great ape in primatology. This graph was compiled on the basis of a bibliometric research and shows the relative percent of articles for each great ape species in five time periods (Hebert and Courtois 1994: figure 1)*

The feminist critique

As early as 1971, Sally Linton published an article entitled 'Woman the Gatherer: male bias in anthropology'. In it, she expressed her severe dissatisfaction with theories of human evolution such as put forward by Washburn and Lancaster (1968) in the *Man the Hunter* volume because they showed 'a strong male bias in the questions asked, and the interpretations given' (Linton 1971: 37):

So, while the males were out hunting, developing all their skills, learning to cooperate, inventing language, inventing art, creating tools and weapons, the poor dependent females were sitting back at the home base having one child after another (many of them dying in the process), and waiting for the males to bring home the bacon. While this reconstruction is certainly ingenious, it gives one the decided impression that only half the species—the male half—did any evolving. (42)

From this criticism, she developed an alternative interpretation in which food sharing, the origin of family, tool use, more complex social organization, complex communication and increased brain size were not the result of male hunting but of female gathering and maternal care for the young. To her, the mother-infant bond formed the base of the family, particularly since the period of infant dependency had lengthened due to increased immaturity at birth. Sharing started with mothers providing food for their long-dependent offspring. A sling for carrying

infants and a container for carrying fruits were the first tools instead of weapons. This was in a nutshell the whole theory which later feminist anthropologists would expand. Interestingly, chimpanzees were not mentioned in Linton's paper, although she did indicate some general resemblances between humans and living nonhuman primates which included close mother-infant bonds and affectionate relationships. Just like the hunting hypothesis had preceded the baboon studies, the kernel of Woman the Gatherer was there before the chimpanzee model emerged.

A much more elaborate argument than the twelve pages of Linton's seminal article was presented in the book *Female of the Species* (1975) by two American anthropologists, Kay Martin and Barbara Voorhies. The book was largely a reply to Lionel Tiger's *Men in Groups* (1969) which we encountered in the context of baboon studies. Martin and Voorhies (1975: 135) had 'several reservations about Tiger's choice of savanna baboons as models for human society': early hominid ecology might have been different, the link between ecology and behaviour was not straightforward, other baboon species also lived in forested areas, etc. They write: 'Lionel Tiger's choice of the savanna baboon as a model of early human behavior has colored his view of human evolution and of present-day societies' because it introduced 'sharp behavioral differences between the sexes' (138) and 'the equation of male aggressiveness with sexual competition' (170). Instead, they suggested that parallels be sought with the apes 'which are more like humans anatomically, genetically, biochemically, and in susceptibility to particular diseases than are baboons' (140). Chimpanzees were to be preferred over gorillas because they showed also many behavioural similarities:

If a single nonhuman primate group with the greatest relevancy for studies of human evolution is sought, chimpanzees appear to be the most highly qualified [...] there are more similarities in the overall trends of chimp and human evolution than with baboon and human evolution. We feel that these broad similarities between chimps and human outweigh the similarity of common habitat between baboons and early humans. (141)

Chimps triumphed over baboons and ecology was no longer a relevant similarity. The sheer number of similarities between chimps and humans had paled the previously all-important criterion of sharing a terrestrial lifestyle in the savannah. The authors proceeded with an alternative view of human evolution, similar to the one suggested by Linton, where women took on a more central role. Interestingly, they did appeal to Jolly's seed-eating hypothesis to substantiate their claim that hunting had not been essential in the earliest phases of human evolution.

With the work of Martin and Voorhies we see how the critique on Man the Hunter widened to a critique on the baboon model and how the alternative of Woman the Gatherer was substantiated with an invocation of chimpanzees. Martin and Voorhies had not studied chimps in the wild, nor had Linton, or Tanner and Zihlman (which I will discuss hereafter); their acquaintance with chimpanzees came from publications of fieldwork. Although we find references to Itani's research group in Mahale, no one has been more responsible for the representation

of chimpanzees to the outside world than Jane Goodall. Her observations of the Gombe chimps have been going on for nearly forty years now and have been paradigmatic for much research on great apes. *In the shadow of man* (1971), Goodall's first popularizing book, was translated into 48 languages, making it the best-read work of non-fiction on any nonhuman primate in the world. Though not an outspoken feminist herself, her often anecdotal accounts of an animal society consisting of individual animals with life histories, friendships and feelings of their own certainly appealed to anyone searching an alternative to the dry, mechanistic descriptions of belligerent baboons (Goodall 1986). Even Goodall's observations of interspecific aggression, infanticide and cannibalism could not change that essentially peaceful picture of chimpanzees in the 1970s.

Nancy Tanner and Adrienne Zihlman, the joint venture of a cultural and a physical anthropologist with a shared interest in human evolution and second-wave feminism, became the most vociferous proponents of the chimp model. In a stream of publications which started in 1976, they canonized the feminist alternative for Man the Hunter into the textbook version as we know it today (Tanner and Zihlman 1976; Zihlman and Tanner 1978; Tanner 1981; 1987; Zihlman 1978; 1981; 1985; 1990; 1992).³⁶

Next to Goodall's fieldwork, biomolecular evidence equally contributed to that theory (Tanner 1987). Immunological studies, comparisons of protein sequences and comparisons of genetic material all converged on what anatomists already assumed since Huxley: that the African apes, particularly chimpanzees, are closer to humans than any other primate. In the 1970s, the figure of 99 % of genetic identity was often given; in recent years most authors have been willing to give in one percent, but the result is still rather impressive. (It seems as if '99 %' is a magical figure in the rhetoric of evolutionary anthropology since this was also the figure invoked by the Man the Hunter-supporters to indicate the percentage of their time on earth that humans had been dependent on hunting.)

A more theoretical impetus came from the nascent field of sociobiology. The father of sociobiology, E.O. Wilson (1975) had argued that apart from the classical Darwinian mechanisms of natural selection and sexual selection, kin selection played an equally crucial role in evolution and helped to explain such ostensibly incomprehensible behaviour like altruism. From Wilson, the chimp modellers borrowed the notion of parental investment (or kin investment) and followed a suggestion on sexual selection made by Trivers that 'the sex investing the most energy in its offspring is the sex that chooses its mates and thus influences the gene flow into the next generation' (Zihlman 1981: 88).³⁷ In other words, since females made the largest parental investment, they would steer sexual selection, not the males. In trying to explain canine reduction and the loss of sexual dimorphism, Tanner and Zihlman believed that 'females were choosing precisely those males who were friendly, nurturing, tool-using, and willing to share food' (Zihlman 1981: 96). Even if canine reduction was a process occurring in the male body, females had been responsible for it. No matter how ingenious, it is hard not to hear the

36 For a particularly vivid and balanced history of Woman the Gatherer and Zihlman's role therein, see Haraway (1989: 331-48).

37 This idea is central to their hypothesis and can be found in all their articles (cf. Tanner and Zihlman 1976: 589; Tanner 1981: 158-67; Tanner 1987: 14; Zihlman 1985: 369).

echoes of women's sexual emancipation in the 1970s in lines like 'females may have come to prefer to mate more often with males who kissed effectively than with those who growled at them and displayed large canines' (Tanner 1987: 14) or 'females probably had sex more frequently with those males who were around often, playing with offspring, helping in protection, occasionally sharing meat and foraged plants, and who were generally friendly' (Tanner 1981: 164). Despite the androcentric bias they pointed out in much earlier anthropology, their projection of the ideal seventies man into the Plio-Pleistocene was no less value-free.

Another source of inspiration for Tanner and Zihlman was R.B. Lee's ethnography of the !Kung. More than anyone else, Lee had demonstrated at the Man the Hunter conference that hunting and meat-eating played only a relatively minor role in a foraging society (Lee 1968). (Zihlman (1981: 91) went even so far to call the resulting book a 'misnamed volume'.) On another conference in the late 1960s, Lee had also argued that the first tool must have been a carrying device: 'a softened animal hide, a bark tray, a broad leaf, or a crude net', in any case something perishable, since it had never been excavated (1979: 491). He believed that such carrying device was 'the essential prerequisite, the sine qua non, of human economy' which 'made possible a human way of life' (Lee 1979: 491). Even if Lee was not clear whether it functioned in contexts of hunting, gathering or just as a child sling after humans had lost their body hair (to which nonhuman primate infants normally cling), the idea that another tool than a hunting weapon played an evolutionary function was grist on the feminist mill.

A dissatisfaction with androcentric scenarios of human evolution based on hunting, a critique of the baboon model, the availability of rich observations on chimpanzees, the biomolecular evidence of a close affinity between chimps and humans, the notion of female sexual selection cast in sociobiological terms, the importance of gathering in a contemporary foraging society, and the women's movement of the 1970s, all these were the axes which converged on the gathering hypothesis of Tanner and Zihlman (cf. Zihlman 1987 for a similar retrospect). They erected a two-staged model of human evolution: first, there was an *ancestral* population of hominids between 6 and 4 million years ago of which only few fossils had been found; second, a *transitional* population emerged when the ancestral population moved into the open savanna and developed into australopithecines between 4 and 2 million years ago. The first ones lived shortly after the hominid-pongid divergence and could therefore be represented by extant chimpanzees; the second ones had to be reconstructed on the basis of imagining how chimpanzees would behave in an open habitat following the principles of female sexual selection, gathering economy and increased infant dependency. Not surprisingly, all the quintessential human traits which were previously thought to be the result of male hunting were now seen as the product of female activities. Gathering was the prime mover and caused sharing (with offspring), tool use (digging sticks and containers), and bipedalism (required for gathering with a container). In their view, women were the 'primary socializers'; the mother-offspring bond, not the male bond (Tiger 1969), was the basic social unit; and females, as suggested before, were the ones who choose whom to have sex with—and thus determined canine

reduction and decreased sexual dimorphism. Economically, technologically, and anatomically, it was women who had determined most of human evolution.

Tanner and Zihlman's evolutionary narrative did not greatly differ from Sally Linton's speculations whom they explicitly acknowledge (cf. Zihlman 1981: 369). Originality was perhaps less the hallmark of their oft-repeated argument than their capacity to draw together several lines of argument, rather eclectically sometimes, into a single hypothesis. Their alternative was, nevertheless, quite problematic. Whereas they had effectively laid bare the androcentric bias in many earlier speculations, their own reconstructions were hardly less speculative (despite the affirmative tone of their prose). In fact, they just presented an inversion of the earlier models where women took over the responsibility of the evolving business. Hominization thus became 'feminization' (Stoczkowski 1996: 281). Frances Dahlberg, editor of the volume *Woman the Gatherer* (1981: 27), had hoped that with the end of the hunting hypothesis, grandiose origins stories would be over as 'heroic qualities seldom come into play in securing protein from catfish, termites, snail, gerbils, and baby baboons. But what is lost in drama', she continued, 'is gained in diversity and complexity.' Her hopes notwithstanding, *Woman the Gatherer* became an equally heroic, one-sided and unilinear tale of human origins.

Feminist anthropologists crusaded not so much against the empirical implausibility of the hunting hypothesis as against its ideological undesirability. This is not to say that the gathering hypothesis was only an ideological construct as opposed to a more objective hunting paradigm, or that the former was bad science and the latter good. All science is political, and certainly primatology (Haraway 1984). So-called external influences are not necessarily distorting, but can bring in new and promising perspectives: pre-judgements often provide fruitful angles of observation, they are in fact what makes science possible. What is most objectionable in Tanner and Zihlman's writings, however, is not so much their feminist bias (this in fact allowed them to detect androcentrism) but their objectivist pretensions (the affirmative tone, the lack of doubt, the obliteration of speculation) in spite of their overt political agenda. Haraway (1984: 96), though sympathetic to the feminist movement, rightly says that 'struggles for feminist science cannot proceed only by writing the tales one wants to be true'.³⁸ *Woman the Gatherer* was first of all 'a contentious marriage of Euro-American feminism and biological humanism' (Haraway 1989: 228). Reading through the feminist alternatives, one gets the impression that all these several lines of evidence were only selected to corroborate the basic hypothesis. The fact that Linton's initial, chimpless gathering hypothesis did not greatly differ from Tanner and Zihlman's full-fledged chimp model, leads one to suspect that the chimps were largely there for ornamental reasons. 'In terms of the Gathering Hypothesis *per se*', Tanner once wrote, 'the Chimpanzee Model is not essential' (1987: 12).³⁹

38 Haraway's feminism is obviously more 'third wave' than 'second wave'. Whereas Tanner and Zihlman were exponents of the second-wave combative women's movement, Haraway better articulates the more recent post-modern form of critical deconstruction (cf. Van Reybrouck 1998b).

39 She continued, however, by defending that the chimpanzee model was useful beyond the assertion of gathering as the economic base.

Of course, this criticism is written in an age where gender issues are high on the agenda so that the position taken here can perhaps not fully come to the terms with the ubiquity and tenacity of the hunting orthodoxy with its chauvinist connotations nor the aspirations felt by many for an alternative narrative of human origins. Man the Hunter had been cast at a time when only very few women were active in primatology. Indeed, it was only in the full 1970s that the sexual demography of the discipline showed a marked increase in female researchers in the field, even to the extent that primatology is now sometimes heralded as a textbook case of feminized science (Haraway 1984; Longino 1990: 103-11; Morell 1993; Fedigan 1994; Fedigan and Strum 1997). It is no coincidence that an awareness of male biases occurred simultaneously with a growing presence of women in primatology. Be that as it may, it is still surprising that at the same time that in archaeology speculation was abolished and subjected to a severely falsificationist and often fecund programme in the work of Isaac and Binford (Chapter 3), speculation could flourish as never before in the study of human origins. Despite their divergent appraisal and use of speculation in their work, Binford, Tanner and Zihlman sided in the rejection of hunting in an early phase of human evolution. For Binford, early hominids had been scavenging, for feminists they had been gathering and for Jolly, as we have seen, they had been eating seeds. The deficit of the hunting hypothesis was obvious, but the place of speculation in it varied enormously. Perhaps this explains why the gathering hypothesis, though often reiterated by its authors and referred to by others as an example of feminized science (Longino 1990: 103-11), never received much feedback in the form of intellectual debate or overt resistance. The reaction was one of repressive tolerance. Bitterly reflecting on this absence of debate, Zihlman (1987: 16) drew a comparison with an odd game of tennis: 'After a great power service, your opponent takes one look at the ball, turns away, and walks off the court, refusing to return your serve.'

A perfect analogy

Even if American feminist anthropology, Woman the Gatherer and the chimp model were largely intertwined (Tanner and Zihlman's seminal writings all appeared in feminist publications), it would be erroneous to assume that the chimpanzee model was confined to feminists only. Others with less overt political agendas equally turned to the chimps (Goodall and Hamburg 1975; Reynolds 1976; McGrew 1981), though they were outnumbered by the zealous publishing of Tanner and Zihlman. In this section I want to analyse the structure of the analogical reasoning in all these texts. Since they resemble each other considerably, I will discuss them as a whole and quote rather extensively from them. Feminists and non-feminists alike reacted against the previous baboon model and considered the chimpanzee to be a better alternative. All these authors equally attempted to outline a new theory of human origins. They shared the same ambition, turned to the same conceptual tool and, despite their interpretative differences, went through the same analogical steps of observing similarity, establishing relevance, predicting similarity and building on that prediction.

1) At the outset of any chimpanzee model lies the fascination with the intriguing similarities chimps and humans demonstrate. It was because of the greater number of resemblances that Martin and Voorhies (1975: 141) preferred the chimp over the baboon. Concurring with that view, Goodall and Hamburg (1975: 15) wrote:

The chimpanzee, however, is a much closer relative of man than the baboon. This is suggested by many lines of research into the biochemistry, physiology, anatomy, and behavior of the chimpanzee. In all these areas there are striking similarities between the chimpanzee and ourselves.

Others spoke of a ‘close affinity’ (McGrew 1981: 36) or ‘intimate biological ties’ (Tanner and Zihlman 1976: 591) and all mentioned the various forms of similarity between chimpanzees and modern humans. Tanner (1987: 7) named both species ‘extremely similar and evidently closely related’ while Reynolds (1976: 68) argued that chimpanzees were physically ‘not so far removed from the ancestral Dryopithecines’.

2) This similarity, the argument continues, is not incidental but the result of a phylogenetic closeness between both species. On the basis of anatomical resemblances, Victorian evolutionists like Huxley had already concluded that both must have descended from a common ancestor. More recent molecular evidence confirmed this position and tried to date the moment of divergence from the last common ancestor; in general it was calculated to be somewhere along 5 million years ago. As a consequence of the biomolecular revolution, phylogeny became more important than ecology. Washburn and DeVore, Schaller and Lowther and many others could still believe that ecology shaped behaviour, but Zihlman and Tanner (1978: 171) rebutted that ‘ecological factors can influence but not determine social structure’:

Models based on particular adaptations, such as living in a savanna habitat, cannot replace a more comprehensive model that includes not only ecological and behavioral factors, but a common, recent evolutionary history—as the chimpanzee model does.

Ecological analogy as a criterion of relevant similarity thus made room for phylogenetic homology.

3) Because humans have so dramatically evolved since the hominid-pongid split (as the fossil record aptly testifies) and since nothing is known about chimpanzee evolutionary history, it is assumed that chimps are much closer to the last common ancestor. They were, therefore, used as living models for it. This crux of the whole argument can be illustrated with many quotes. Goodall and Hamburg (1975: 30) referred to ‘a hypothetical group of chimpanzees, or chimpanzeelike creatures [...] that are undoubtedly similar in many ways to our own ancestors’. McGrew (1981: 59) reasoned from ‘the chimpanzee-like early hominid’. Reynolds (1976: 68) argued: ‘The model I want to suggest [...] for early hominid society is the chimpanzee model.’ And Tanner and Zihlman, as could be expected, defended

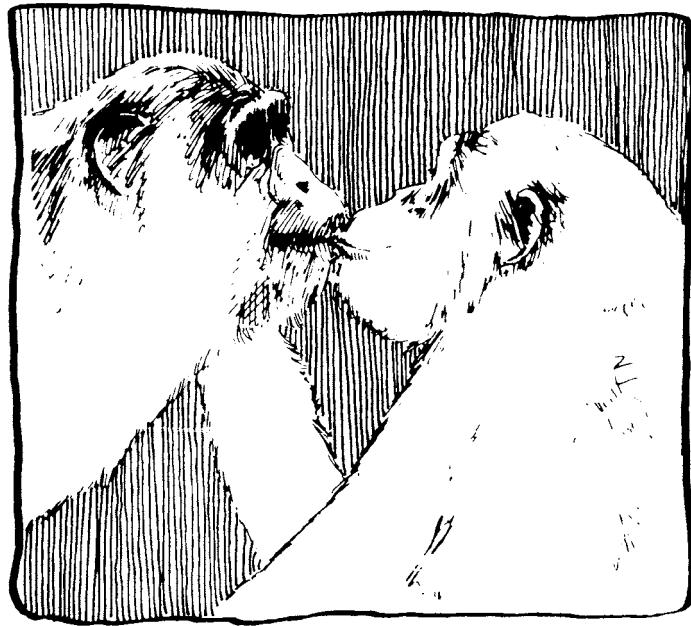


Figure 18. The frontispiece of Tanner's *On Becoming Human* (1981) shows two female chimpanzees—a curious choice for a book on human evolution. The author, however, acknowledged the illustrator for having 'translated the concepts and data into visual form' (1981: xvii). Following the argument of the book—which was the longest defense of a primate model ever—the reader was eventually forced to admit that we had indeed become human from an ancestral stratum of chimpanzee-like friendliness and sociability. The gently kissing, peace-making chimps in the frontispiece did not only summarize her theory; they were also an indirect visual reply to DeVore's widely-disseminated photograph of the aggressive, threatening, barking, canine-displaying, solitary adult male (cf. Figure 15).

this point emphatically (Tanner and Zihlman 1976: 591, 596; Tanner 1981: 58, 65; 1987: 6, 15, 24; Zihlman 1987: 14; 1992: 415, 417). At one point, they even simply stated that 'living chimpanzees represent the kind of population from which we evolved.' (Tanner and Zihlman 1976: 588). The argument was even put in visual form: the frontispiece of Nancy Tanner's 1981 monograph *On Becoming Human* depicted a line drawing of two female chimpanzees kissing each other, suggesting the importance of female sociability and reconciliation (figure 18). Coincidence or not, at this time the author started to sign her publications with her second name: Nancy Makepeace Tanner. The numerous publications and extreme phrasing of the Tanner and Zihlman tandem should not obliterate the widely-felt appropriateness of the chimp model. To date, the chimpanzee is still regarded the most popular candidate for the best model by many students of primate behaviour (cf. Infra).

4) After making the point that chimpanzee life resembled our ancestral existence before entering the savannah, most papers continued with lengthy descriptions of the social life of chimps—apparently believing that these pieces of natural history prose could fill in the void of the missing link, that these descriptions were actually arguments. Reynolds (1968: 69-72) did it, McGrew (1981: 37-57) did it,

but the most extreme case is Tanner's *On Becoming Human* (1981): after defending a chimp model in Chapter 3, the next three chapters, all entitled 'Chimpanzees as a model of the ancestral population', are strictly devoted to a description of chimpanzees' locomotion, tools, diet, social organization, interaction, mental capacities, communication, and 'sociation'. In this, she follows the same evocative procedure we have encountered in the textbooks on baboons in the early 1970s and which we will find in Sollas' prose on Tasmanians, Australians, Bushmen and Eskimos. The chimp model was not a starting point for further explorations and inferences, but served as an end point. Once the model defended, it sufficed to simply describe how chimps behaved. Chimpanzees presented a living and tangible image of a remote past and the metaphor of such visualization became one of the favourite rhetoric devices. Chimpanzees made it 'possible to *visualize* how the hominid line could have arisen' Tanner and Zihlman held (1976: 596, italics added). For Goodall and Hamburg (1975: 30), their hypothetical group of chimpanzees gave '*a picture* of a group of beings that are undoubtedly similar in many ways to our own ancestors' (Goodall and Hamburg 1975: 30; italics added). No matter that all authors warned that chimps were of course not really living ancestors (Reynolds 1976: 68; McGrew 1981: 37; Tanner 1987: 26), most continued unhindered by these cautions. The invariable defence of such models was that they had a 'heuristic value'—they could at least suggest new ideas. Now few words are so abused in primatological arguments as the word 'heuristic', because often it only serves to claim something which the author cannot really prove; it gives a licence to turn an idea into a fact without further ado. Whenever a step of one's argument is poorly elaborated, it suffices to call it 'heuristic' to move on. Chimps were said to be only heuristic sources, but they continued to be holistically projected into the past. Despite such formulaic apology, they effectively functioned as our beginnings, the *ab Urbe condita* of humanity.

5) 'By using the conservative chimpanzees as a model of the ancestral population, we gain a starting point,' Tanner wrote (1981: 65). After having projected chimpanzees into the past, chimp modellers sent them 'back to the future' through a different habitat, this time the savannah. All authors developed the experimental mind game of imagining how a group of chimpanzees would survive in the savannah, hoping to find out how early hominids had done. Goodall and Hamburg (1975) steered their 'hypothetical group of chimpanzees' into the open savannah and came up with hunting, Tanner and Zihlman (1976) with gathering and McGrew (1981) with cognitive maps and host of other assets. On the basis of limited evidence and with a malleable actor like the chimpanzee on stage, everyone could construct his or her own scenario according to personal preference. Though nearly all authors admitted the speculative character of their 'just-so story', most of them defended the necessity of such speculation. McGrew (1981: 58, original italics) was most honest about it: '*Although the arguments are speculative, I shall present them definitively, for the sake of convenience.*'

What happened with the chimpanzee model is a classical example of what logicians have called 'the fallacy of the perfect analogy' (Fischer 1970; Wylie 1985):

The fallacy of the perfect analogy consists in reasoning from a partial resemblance between two entities to an entire and exact correspondence. It is an erroneous inference from the fact that A and B are similar in some respects to the false conclusion that they are the same in all respects. One must always remember that an analogy, by its very nature, is a similarity between two or more things which are in other respects unlike. A "perfect analogy" is a contradiction in terms, if perfection is understood, as it commonly is in this context, to imply identity. (Fischer 1970: 247)

The chimpanzee model was defended on the basis of similarity made relevant through phylogenetic closeness. And although in the case of chimps and modern humans this similarity (both in number and in variety) and phylogenetic affinity is unmistakable, it can never entail that the source analogue be projected *as a whole* on the target. This is even more so since the target consisted of fossil hominids, not modern humans; it must not be forgotten that the observed similarity between chimps and hominids was asserted only indirectly through the resemblance with *modern* humans. Analogical reasoning consists of the transfer of *certain* attributes of the source to the target. In the case of the chimpanzee model, the similarity was so much stressed that the relation of analogy became one of identity. This is to be taken very literally: people did indeed assert that 'living chimpanzees represent the kind of population from which we evolved' (Tanner and Zihlman 1976: 588). The chimpanzee model did not so much serve to draw inferences about the past as to represent an image in flesh and blood of how it might have been. As a result, chimpanzees served as contemporary ancestors just like the savages had done in the schemes of Morgan, Tylor and Sollas. Whereas defending the source analogue would normally be a starting point for further inquiries, in the case of the chimpanzee model, as in nineteenth century evolutionism, it was an end point. Once the similarity assessed, most authors simply went on to give a description of chimpanzee social life as if it substituted reasoning about early hominid society. A perfect analogy erroneously assumes that partial similarity implies total identity, and becomes, therefore, tautological. The problem, however, is that one never knows anything else about the last common ancestor that was not already known about the common chimpanzee; the uniqueness of the former can never be known. The distance between the two has been obliterated by projecting the one onto the other. Rather than a window to the past, the chimpanzee source has turned into a closed shutter.

This projective method based on overall similarity had two important consequences: the neglect of differences and the restriction to older periods (Tooby and DeVore 1987: 187). Indeed, the chimpanzee model in general stressed similarities at the expense of differences. With the notable exception of Goodall and Hamburg (1975), most authors remained rather silent about the obvious differences. Whereas the earliest workers on baboons had been intrigued by the differences they presented to modern man, whereas carnivore and gelada modellers had tried to deal with the many dissimilarities between their source and target, now any discussion of difference was anathematized. Zihlman concluded a recent paper with the remark that 'it is the similarities between chimpanzees and human,

rather than the differences, that need to be emphasized in order to understand this earliest stage of hominid evolution' (1990: 194).

The fact that similarity was the all-important criterion also explains why chimpanzees were never used for more recent stages of human evolution, like *Homo erectus*, where the distance between source and target is obviously bigger. 'Women in evolution, part II' (Zihlman 1978), the sequel to the seminal paper by Tanner and Zihlman (1976), dealt with the later australopithecines and *Homo erectus*. Not surprisingly, chimpanzees are entirely absent from the speculations here. Just like in the early seventies' textbooks, chimps were thought to represent the earliest hominids in the first act of hominization—the reason no longer being an ecological analogy but an overemphasized similarity.

The seductiveness of similarity

Compared to the careful analogical reasoning we encountered in the carnivore and gelada models, the chimpanzee model was logically far less refined. It pivoted around the notion of phylogenetic homology which assumes that genetic similarity automatically entails behavioural similarity. Kortlandt (1986: 126), however, called the assumption of homology of behaviour 'a risky inference scientifically':

One may all too easily apply such circular arguments as: "Because chimpanzee and man are closely related, similar elements in their basic behaviour are probably homologous and, therefore, date back from the common ancestor." These and similar errors of reasoning occur abundantly among chimpanzee researchers as well as among anthropologists.

Nevertheless, the model's proponents tried to narrow the distance between source and target by enumerating as many similarities as possible—even to the extent that the analogue sides conflated with each other. In the end, the chimpanzee model of the late 1970s and early 80s obfuscated rather than elucidated the last common ancestor. Phylogenetic affinity is not taxonomic or behavioural identity.

In contrast to the social carnivores and the geladas, the chimpanzee was of course a less remote source—it presented a 'manifest' rather than an 'imported analogue' (Leatherdale 1974)—but eventually this similarity was rather disadvantageous than beneficial. This is not to deny that chimpanzees stand indeed much closer to the last common ancestor than a gelada baboon—of course they do—but the problem is that there is no way to know how *different* they are from the last common ancestor and how much they have evolved since the hominid-pongid divergence. Most scholars have assumed that, whereas hominids had evolved dramatically over the course of the last 4 million years, chimpanzees were much more static, both anatomically and behaviourally. Tanner called them 'conservative'! The fact that absolutely nothing was known about chimpanzee fossil history in the last 5 million years was more of a profit than a problem to chimpanzee modellers: it gave them a licence to assume that chimps had not changed.

Yet chimpanzees have changed. It is now accepted that they manifest regional and cultural traditions and that these variations in behavioural patterns can emerge and spread rather rapidly (McGrew 1992; De Waal and Seres 1997). Furthermore,

recent research has acquainted us with the social and sexual life of the bonobo. The chimpanzee–bonobo split is calculated on the basis of genetic divergence to have occurred as recently as 2 million years ago. Apparently, it took only two million years to produce the substantial differences in the behaviour of both species (De Waal and Lanting 1997: 23–47; but see Stanford 1998)! No matter whether it was the bonobo or the chimpanzee who moved away from the ancestral stock (or both), this lesson teaches that chimpanzee species are susceptible to rapid, evolutionary change.

These are of course recent findings and it is not fair to criticize past knowledge in history of science with new evidence, especially if this evidence comes from a species which was still largely unknown in the late 1970s. Yet even at the moment this knowledge on bonobos became available, it was not summoned to rebut the chimpanzee model but cast instead into a new and even more radical version of it: the bonobo model.

Bonobos

Bonobos were discovered only very recently compared to the other great apes. Although one of the two chimps Robert Yerkes kept as pets was clearly a bonobo, the animal was not recognized as belonging to a different taxon. It was only in 1929 that bonobos were first raised to the subspecies level (*Pan satyrus paniscus*, Schwarz 1929) and a few years later to the species level (*Pan paniscus*, Coolidge 1933). By then, chimps had been known and described for about three centuries, orang-utans for two centuries and gorillas for one. This tardy appearance on the zoological scene explains why until very recently so little was known about this animal. If anatomical studies on skins and skeletons raised already considerable taxonomic uncertainty, how much more elusive was the behaviour of so timid an animal! Political unrest in Congo, the only country where bonobos occur in the wild, further frustrated the possibility of setting up long-term research stations. Indeed, up to well in the 1980s, very little was understood of the bonobos social life in the wild (Susman 1984; Kano 1992; De Waal and Lanting 1997). It is therefore not surprising that the first bonobo models for human evolution were by and large based on anatomical and biomolecular research rather than behavioural studies. At the outset the scope of this analysis was limited to behavioural models. The bonobo model, however, cannot be neglected as it belonged to the long-standing discussion on primate modelling, had developed out of the previous chimp model (its main proponent was Adrienne Zihlman) and, most importantly, gave rise to one of the most interesting discussions on the use of models in the history of primatology.

The disputed bonobo model

After the sweeping statements of Woman the Gatherer had been made, Zihlman turned back to the intricacies of her own discipline—physical anthropology—by conducting research on the skeleton of bonobos. In two papers co-authored by Douglas Cramer and published in the same year, she sought to understand the

skeletal differences between chimps and bonobos and the extent of sexual dimorphism in the latter (Zihlman and Cramer 1978; Cramer and Zihlman 1978). She concluded that bonobos were not simply a scaled down version of common chimpanzees and that sexual dimorphism, especially in the cranial and facial regions, was absent. Though dealing with localized morphological problems, in several places the papers suggested more broadly that ‘pygmy chimpanzees’ (as they were still called then) could be ‘the most similar of living primates to the common ancestor of man and African apes’ (Zihlman and Cramer 1978: 86). Nearly half a century earlier, Coolidge (1933: 56) had already assumed that the bonobo ‘may approach more closely to the common ancestor of chimpanzees and man than does any living chimpanzee hitherto discovered and described’; Zihlman drew upon that prediction in her morphological comparison of bonobo skeletons with australopithecine fossils (1979).

This hint became the main theme of an article published in *Nature* in 1978 under the unmistakable title ‘Pygmy chimpanzee as a possible prototype for the common ancestor of humans, chimpanzees and gorillas’. The physical anthropologists Cramer and Zihlman (who was the first author) were sided by biomolecular researchers Cronin and Sarich in their claim that ‘pygmy chimpanzees present a general pattern from which other African hominoids could have developed’ (Zihlman et al. 1978: 744–5). The argument was supported by four converging lines of evidence: biochemical research supplied a cladogram in which the two chimpanzee species split apart ca. 2 Myr ago (well after the hominid-pongid divergence dated at 4 Myr); morphological studies showed that pygmy chimps were not smaller than chimps but simply more ‘generalized’; behavioural observations in captivity and in the wild, even if few, suggested that bonobos were highly intelligent, socially flexible and inclined to bipedalism; palaeontological comparison indicated many similarities between skeletons from bonobos and gracile australopithecines (cf. Zihlman 1979). These taken together led to the conclusion that of all living apes the bonobo had retained most of the ancestral character states and could therefore be seen as prototypical (figure 19). Whereas the chimpanzee had diverged away from the ancestral stem to become what they are now, the bonobo was thought to have changed less so that it offered ‘the best prototype of the prehominid ancestor’ (Zihlman et al. 1978: 744). The gist of this argument was again based on notions of similarity: biochemical similarity to modern humans, anatomical similarity to australopithecines and vague behavioural resemblances with the human pattern. The idea was not so much to infer certain attributes on the basis of shared attributes (observed similarities), but to present a life-image of this common ancestor of which no fossils were known. As was the case with the chimpanzee model, once the bonobo was sufficiently defended as a suitable source analogue, the inquiry came to a halt. Rather than serving as a base for further inferences, the bonobo model seemed to satisfy the need for an immediate image. It is not accidental that in this context the metaphor of visualization and imagery occurs. Zihlman (1996: 301) wrote that the bonobo had been ‘useful in visualizing the transition from quadrupedalism to bipedalism’.



*Figure 19. Morphology formed one of the key arguments of the bonobo model. This illustration appeared in *Nature* to suggest general similarity between a bonobo and an australopithecine skull; critics said it forced this similarity (Zihlman et.al. 1978: figure 2)*

Compared to her other work, these papers by Zihlman (and her co-authors) were rather technical and more neutral in tone. The explicit feminist agenda and its speculative offshoot, the gathering hypothesis, were entirely absent from them.⁴⁰ Although bonobo society would eventually turn out to be matriarchal and based on female coalitions—observations in line with the feminist claim of female centrality—this was still unknown in the late 1970s.⁴¹ The crux of the bonobo model lay in biochemistry and morphology, not in ethology. Whereas the chimpanzee model was proposed in the first issue of a then unknown feminist journal, the bonobo model saw the light of day in the pinnacle of academic publishing, the weekly *Nature*. Despite its more academic appearance and granted scientific credentials (or perhaps precisely due to these), the bonobo model was severely criticized from several angles.⁴²

In 1981 three papers appeared which replied independently to the *Nature* paper (McHenry and Corruccini 1981; Johnson 1981; Latimer et al. 1981). The authors came from disciplinary backgrounds as diverse as morphology, mammalian palaeontology and palaeoanthropology, and their criticisms appeared in periodicals

40 Only in a more popular paper like Zihlman's fanciful interview with the fossil hominid 'Ruby' (named after the Rolling Stones' *Ruby Tuesday...*), does the bonobo model and the gathering hypothesis intermingle (Zihlman and Lowenstein 1983a).

41 More recently, Zihlman (1996: 301) has stressed the importance of the behavioural findings of 'the female-centric features of their society'.

42 In fact, articles in *Science* and *Nature* are perhaps by definition the most heavily criticized scientific publications (cf. the storm of protest released by Lovejoy's 1981 *Science* paper). Authors publishing in these weeklies certainly live up to one of Oscar Wilde's less famous aphorisms: 'there is only one thing worse than being talked about, that is not being talked about'.

such as the *American Journal of Physical Anthropology*, *Current Anthropology* and the *Journal of Human Evolution*. Henry McHenry and Robert Corruccini were both physical anthropologists working on primate morphology who after comparing bonobo skeletons with chimpanzees, modern humans and fossil hominids found themselves disagreeing with Zihlman and her co-authors on nearly all points. Firstly, although they acknowledged the differences between the bonobo and the chimpanzee, almost all differences they believed were simply ‘due to size differences and the effect of allometry’ (McHenry and Corruccini 1981: 364). Secondly, although the bonobo was slightly more like modern humans in some respects (such as limb proportions), in other respects (like shoulder and hip morphology) the chimpanzee was closer—the bonobo not bearing any nearer resemblance to humans. Thirdly, although there were some similarities between bonobos and fossil hominids, ‘the special resemblance generally disappears with allometric correction’ (364). Point by point, the authors undermined Zihlman’s claims by emphasizing the effects of allometry on the skeletons, i.e. the evolutionary process whereby specific morphological differences are the result of overall scaling rather than localized adaptation. Metric differences between bonobos and chimps would be caused by such process since ‘*Pan paniscus* is an allometrically scaled version of *P. troglodytes*’ (361). What they actually did in logical terms was adding a notion of relevance to the similarities and dissimilarities by invoking allometry. Yes, there were indeed differences between bonobos and chimps but these were not relevant considering the effect of scaling. And yes, there were similarities between bonobos and hominids but these were equally irrelevant and disappeared if you accounted for allometry. Allometry was the causal pattern which determined the relevance of observed (dis)similarities. Consequently, the last common ancestor did not find a privileged prototype in the bonobo any more than in the chimpanzee; it simply had ‘some special features in common with each of its living descendants’ (364).

The mammalian palaeontologist Steven C. Johnson expanded this critique by incorporating an explanation for the allometric process. In an article in *Current Anthropology* he expressed his doubts as to whether bonobos could really be seen as generalized and therefore reminiscent of an ancestral state. He argued that there was good reason for believing that these animals were recently specialized insular dwarfs instead of generalized hominid prototypes. To him, bonobo morphology showed an adaptation to a more terrestrial form of locomotion than the one seen with chimpanzees. The limited sexual dimorphism and the small size of the mandible and teeth contrasted sharply with the oldest known fossil hominids of that date, *Australopithecus afarensis*. These features were in his view the result of a specific adaptation, not an ancestral relic, in what Johnson calls ‘a “terrestrial” island of tropical forest’ (1981: 364). Just like the small Pleistocene hippopotamus on Cyprus and the shrunk Holocene mammoths on Siberia’s Wrangel Island, bonobos had undergone a process of ‘insular dwarfing’—their ‘island’ being a stretch of tropical forest formed by the Congo river in the north and a more arid environment in the south. Johnson built upon the theory of allometry suggested by McHenry and Corruccini by giving an ecological and biogeographical reason for it: ‘Cranial and dental differences between bonobos and common chimpanzees

may be attributable to allometric effects of dwarfing that occurred in the bonobo's tropical-forest "island" home range' (Johnson 1981: 364). Rather than icons of early humanity, bonobos appeared as specialized adaptations to a relatively small patch of tropical rainforest.

Following *Current Anthropology*'s standard protocol for leading articles, Johnson's paper was followed by an expert discussion. Of the twelve respondents, only two were opposed to his thesis (Zihlman being one of them), four did not speak out and six wholeheartedly supported his view. One of the respondents (D.J. Chivers) even named the bonobo model 'an idea I have always found silly' (365). With comments ranging from palynological arguments to discussions on 'postcanine maxillary tooth area', there is no point in summarizing this multifaceted debate but it might be more interesting to zoom on Zihlman's comments and Johnson's reply. Zihlman reiterated her biochemical, morphological, behavioural and palaeontological argument. While admitting that, biochemically, bonobos are as close to humans as chimps, their limited sexual dimorphism and their 'striking similarities' to the morphology of *A. africanus* still made them a better candidate. 'Finally,' she wrote, 'his argument [Johnson's] that *P. paniscus* is an insular dwarf, though not living on an island and not a dwarf, is purely speculative' (372). Johnson replied in an equally irritable way. The similarities Zihlman had observed with early hominids were of no value, he wrote:

comparative measurement is not comparative anatomy and raw measurements compiled without regard for the specific morphology and adaptations of the forms being compared are of little value. [...] I have obtained identical measurements [to the dimensions in Australopithecus and bonobo] from such disparate forms as Macropus rufus (the red kangaroo, a saltatorial marsupial) and Felis concolor (the puma, a digitigrade carnivore). [...] The "striking" metric similarities noted by Zihlman do not demonstrate close morphological or phylogenetic affinity between hominids and bonobos. (Johnson 1981: 373)

Again, the critique was on the neglect of considering the actual relevance of the similarities found. There was no point in comparing measurements if one did not know what they actually meant. Unlike Zihlman, Johnson did not look for a living, visualizable representative of the common ancestor; much better models could be found among the Miocene hominoids, he thought. A living-primate model was not imperative: 'Zihlman asks if I "believe" that apes and humans had a common ancestor that was like gibbons, chimpanzees, or gorillas. I choose not to believe in hypotheses, but instead to formulate and test them' (373).

If Johnson's words were harsh, the article by Latimer, White, Kimbel, Johanson and Lovejoy (1981) was even more damning. From its title ('The pygmy chimpanzee is not a living missing link in human evolution') to its conclusion, this paper marshalled all possible arguments against a bonobo model. It rejected Zihlman's evidence, criticized her methodology, indicated logical fallacies, refuted her hypothesis with new fossil evidence, appealed to the authority of Darwin and named the use of her illustrations 'selective'. The authors undermined Zihlman's four lines of reasoning: molecular evidence did not favour the bonobo over the chimpanzee; in the absence of fossils, bonobo morphology

on its own could not determine whether the species was generalized or specialized; behavioural information was anecdotal and inconclusive; palaeontological comparison was useless if only body size (instead of form) is included.⁴³ At the end of the paper, the bonobo model looked like a town through which the troops had passed. In logical terms, the arguments summoned by this offensive came down to what other critics had done too: weighing the alleged similarity in terms of its genuine relevance. Either the similarity was accidental and therefore not relevant, or it was forced, or it was non-existent. Accidental: ‘Similarities between hominid and bonobo skulls are fortuitous, illustrating basic architectural parallels instead of close cladogenetic association’ (480). Forced: ‘Lateral line drawings of the toothless Sts 5 cranium and an adult *Pan paniscus* are described as “similar”. [...] Resemblances here relate to the common plan of hominoid cranial structure rather than any special phylogenetic affinity.’ (482). Non-existent: ‘No published biochemical results suggest that *Pan paniscus* bears any special relationship with the hominid clade *or* that it best exemplifies the hominid ancestor’ (479). Instead of an over-reliance on a single living species like the bonobo, the authors with their shared background in palaeoanthropology urged to turn to the fossil record as the prime source of evidence concerning human evolution. Living primates provided ‘no substitute for scientific investigation of the fossil record’ (485).

From McHenry’s careful refutation of the morphological side of the bonobo model over Johnson’s alternative explanation for the patterns observed we have now come to a unmitigated critique against it in the article by Latimer *et alii*, which even evolved into a generic scepticism against any ‘evolutionary scenario based on an extant animal’ (485). While Zihlman once complained about the lack of debate on the gathering hypothesis, now her ‘powerful service’ was returned with equal, or even stronger force. After that triple attack, the debate on the bonobo model turned into a stubborn trench warfare during the following years. On several occasions, Zihlman restated the model though she never mentioned the paper by Latimer *et alii*. Her claim was, however, less affirmative since the lines of evidence only ‘slightly favor a *Pan paniscus*-like model’ (Zihlman 1996: 301):

*In response to these objections, we do not claim that *Pan paniscus* is an ideal prototype in every particular, but rather that, of the living hominoids, this species is probably more like the common ancestor of humans and apes—more so than the common chimpanzee, gorilla, orangutan, or gibbon.* (Zihlman and Lowenstein 1983b: 689)

Her colleague Cronin (1983: 133) believed it was still ‘a viable hypothesis’: ‘If nothing else, it serves a heuristic purpose,’ he wrote. McHenry, on the other hand, took ‘over 20,000 measurements’ to conclude that ‘no extant hominoid has an exclusive claim on affinities to *Australopithecus*’ (McHenry 1984: 203, 218). And so on. People were at pains to be proved right, but the discussion was basically over.

43 Parallel to Johnson’s reference to kangaroos and pumas, the authors claimed that ‘none of the metrics used by Zihlman separate a female black bear (*Ursus americanus*) from the bonobo’ (Latimer et al. 1981: 482).

Bonobo behaviour

At the time the bonobo debate got overheated, an understanding of how this primate behaved in captivity and in the wild was still very much in its infancy. Latimer and his co-authors rightly summarized the contemporary knowledge on bonobo behaviour as ‘inconclusive’, ‘anecdotal’ and ‘poorly documented’ (1981: 481). Not surprisingly, its impact on the debate surrounding the bonobo model was consequently rather limited. Yet at the time the argument raged on in American scientific journals, observers in the African forest were making groundbreaking observations on the social and sexual life of these animals. As so often in the history of primatological fieldwork, the way was paved by the efforts of Japanese scholars. In the early 1980s authors like Kano, Kitamura, Kuroda and Mori published the first results of their observations on the bonobos at Wamba in *Primates*, the English-language journal of the primate research group at Kyoto University. Around the same time, the Irish-South African couple Badrian and Badrian reported intriguing observations from their field station at Lomako. And in North-America, Sue Savage-Rumbaugh’s experiments with the language capacities of the bonobo Matata and its prodigious child Kanzi steered the attention beyond the bones, teeth and molecules of the species. In 1984 the first book on the bonobo was published, an edited volume containing seventeen chapters on the biology and behaviour of the species (Susman 1984). The book was hailed as ‘a sensational contribution to the field of primatology’ with an eminent power ‘as a catalyst for future research on hominoid evolution’ (Dahl 1986: 99).

In the decade following this enthusiastic outcry, the knowledge on bonobos expanded tremendously (Hebert and Courtois 1994). Bonobos were seen to use tools in the wild and in captivity (Walraven, Van Elsacker and Verheyen 1993; Van Elsacker and Walraven 1994; Ingmanson 1996), to hunt, eat and share meat in the wild (Ihobe 1992; Hohmann and Fruth 1993) and to recognize themselves in mirrors (Walraven, Van Elsacker and Verheyen 1995). More than anything else, the tension-regulating function of their abundant sexual life, which includes frontal copulation, homosexual contacts (particularly between females) and other reconciliatory mountings, has received widespread attention (De Waal 1987). A social system based on female dominance and female coalitions coupled with an absence of rigid hierarchies has not ceased to fire scientific and public imagination ever since its discovery. Comparisons and contrasts with the common chimpanzee were repeatedly drawn: more peaceful, more sexual and less chauvinist, bonobos were presented as the late-twentieth version of the noble savage, in strong contrast to chimpanzees who had been observed to commit infanticide, cannibalism and lethal territoriality. De Waal wrote: ‘The chimpanzee resolves sexual issues with power; the bonobo resolves power issues with sex’ (De Waal and Lanting 1997: 32;

but see Stanford 1998 and Vervaecke, De Vries and Van Elsacker 2000).⁴⁴ Book-length publications followed: Kano published a substantial monograph (Kano 1992; translation of his 1986 work in Japanese); De Waal wrote a more popular account which was lavishly illustrated with photographs by Lanting (De Waal and Lanting 1997) and a narrative report of the first Belgian bonobo expeditions appeared (Draulans 1998).⁴⁵

With this eruption of field studies and publications on bonobo behaviour one would expect the bonobo model to be rapidly rekindled, especially since the opinion started to be voiced that hominization might equally have taken place in the forest rather than in the savannah (Susman 1987; Boesch-Achermann and Boesch 1994).⁴⁶ Frans de Waal—one of the principal experts on bonobos—wrote that bonobos with their stronger inclination to bipedal locomotion than chimpanzees ‘look as if they stepped straight out of an artist’s impression of early hominids’ (De Waal 1995: 58). Yet despite this baffling correspondence to expectations of what early hominids might have looked like, De Waal voiced the changed climate of opinion when he refrained from using bonobos as a model:

Rather than favoring parallels with one or the other ape, there is no need to choose between the two. Those who for ideological reasons are inclined to advocate the bonobo as the model of the missing link should realize that evolutionary biology does not permit such selective attention. One cannot nibble on a little piece of it without swallowing the entire pie (De Waal and Lanting 1997: 143).

Against the bonobo modellers, he argued that ‘no extant species can be adopted as model’—on the contrary, ‘the most successful reconstruction of our past will be based on a broad triangular comparison of chimpanzees, bonobos, and ourselves within [a] larger evolutionary context’ (142, 143). Comparing chimps and bonobos was increasingly

44 Stanford (1998) has argued that the differences between bonobos and chimps have been overemphasized: the dominance of studies in captivity have led to an overrepresentation of sexual activity, whereas the amount of observation in the wild has been too limited to report such rare and dramatic behaviours like infanticide. His view may, however, be somewhat ‘chimpocentric’, considering the reactions of bonobo-experts (Parish, Fruth, De Waal in Stanford 1998), but it is certainly the case that chimps have been portrayed as much more aggressive now that bonobos are on the scene (cf. Wrangham and Peterson 1997). The issue will probably not be settled in the near future. Despite the increased interest in wild bonobos, Congo’s political turmoil of the past years has drastically limited the possibilities of ongoing fieldwork in the equatorial forest.

45 The boundary between popular and academic publications has become rather fluid in recent primatology (cf. Haraway 1989: 128–9). All books by Frans de Waal (*Chimpanzee Politics, Peacemaking among Primates, Good-Natured and Bonobo*) are at once writings destined for a lay audience and influential arguments in primatological debates. More than collections of anecdotes, these books present interpretative schemes which escape the rigid statistical requirements of journals like *International Journal of Primatology* and *American Journal of Primatology*. This ensures not only mercantile but also academic success: the popularity of De Waal’s books outside the discipline makes him a voice to be heard within the discipline and vice versa. ‘Popular’ books are nowadays among the most powerful rhetorical devices within primatology.

46 These proposals bring the argument of ecological analogy back in by the back door, after it had been silenced with the decline of the baboon model. Apart from biochemical, anatomical, behavioural and palaeontological similarity, the ecologies of both source and target species would also correspond.

seen as a means to understand evolutionary principles of primate adaptation, and these principles were also felt to be more relevant to human evolution than just projections.⁴⁷

At the very moment when evidence on bonobo behaviour was becoming available, the interest in constructing a single-species model seemed no longer alive (even Zihlman (1996) spent very little attention to bonobo ethology). Why was this? What happened between the initial enthusiasm for bonobos in the mid-1980s and the dismissal of a model-building attempt hardly more than ten years later? Were the bonobos themselves to be blamed for this decreased interest? Not in the least. In fact, they were the ideal stuff for primate modelling; bonobos behaved in ways that went beyond the modellers' wildest dreams. If their behaviour had been well-documented in the 1970s, *Woman the Gatherer* would have been certainly flanked by a bonobo model instead of a chimpanzee one. If not the animals themselves, what else? Perhaps the reason for this decline was already alluded to in the last sentences of Latimer *et alii*'s critique: 'An evolutionary scenario rigidly based on an extant animal like the pygmy chimpanzee suffers from an inherent inability to adjust its parameters in accomodating new types of relevant data' (Latimer et al. 1981: 485). Not just the bonobo, but any single primate species had become doubtful for reconstructing human evolution. Now that bonobos started to be understood, a crisis of traditional modelling emerged.

Bonobos were somehow like the Tasmanians invoked by Tylor at the end of the nineteenth century: they presented the best case for modelling, but they were discovered too late, or at least at moment when criticism against this sort of analogical reasoning began to emerge. Just like the remarks of Boas and Westermarck made the use of the comparative method for projection rather doubtful, a number of primatologists started to reject the tradition of drawing single-species models.

Entrapped by resemblance

Just like chimpanzees, bonobos functioned as source analogues which were projected wholesale onto the target. They were never used for any other stage than the one they were thought to most closely resemble, i.e. the ancestral state at the hominid-pongid split. Similarity was supposed to imply identity, so that the fallacy of the perfect analogy was again committed. And even if, later, the bonobo modellers did 'not claim that *Pan paniscus* is an ideal prototype in every particular' (Zihlman and Lowenstein 1983b: 689) or that 'no living primate is identical in all respects to the hominid ancestor' (Zihlman 1996: 301), *ideally* the best model would still be one which is identical in all respects. Even if bonobos were not ideal, they scored best. The quality of the model was still read off of its degree of similarity.

⁴⁷ According to several authors, the difference between bonobos and chimps had to be explained because of the gorilla-free environment the former lived in. Not having to share the available food resources, bonobos lived in a rich habitat where food stress could not cause competition.

The bonobo model was not so much a device for future research as an end point in itself. Once it was argued that bonobos were closest to the last common ancestor, the argument, again, stopped. Apparently, the aim was not so much to draw analogical inferences or to formulate model-based expectations but simply to give a vivid image (or at least an approximation of it) of our common ancestor. Illustration, not demonstration, was the purpose of the bonobo model. Perhaps this is the best proof that reasoning from identity always becomes tautological: the moment the distance between source and target is said to be close to nothing, little more can be added in terms of explanation.

The debate surrounding the bonobo model, however, marks one of the most interesting episodes in the history of primate modelling because for the first time a single-species model was criticized by contemporary experts. The baboon model had only been rejected after more than a decade, the chimpanzee model met with no resistance at all, but the bonobo model initiated a whole battery of criticisms in the early 1980s. No matter how diverse, the counter-arguments mostly came down to the awareness that ‘similarity is not enough’. Critics of the bonobo model put into practice Copi’s (1972: 358) warning that ‘it should not be thought that there is any simple numerical ratio between the number of points of resemblance asserted in the premises and the probability of the conclusion’. Whereas the chimpanzee modellers were seduced with impunity by the similarity of chimps to modern humans and indirectly to hominids, the advocates of the bonobo model could no longer get away with that. Similarities and phylogenetic closeness notwithstanding, the last common ancestor might still have been very different from what we see today: bonobos have evolved, too. Frans de Waal, for example, has argued that the female coalitions and the female dominance among bonobos are not ancestral relics but aspects of a social adaptation which emerged to reduce infanticide (De Waal and Lanting 1997).⁴⁸

Despite the aspiration of finding a life image of the remote past, the awareness that a species’ behaviour is the result of particular historical and evolutionary processes lay at the base of the rethinking of the conceptual foundations of primate modelling after 1985.

The crisis of traditional modelling

The edited volume *The Evolution of Human Behavior: Primate Models* (Kinzey 1987) is without doubt the most important publication in the history of primate modelling. Based on a symposium at the annual meeting of the American Anthropological Association in Chicago in 1983, the resulting book brought

48 Though rarely made explicit, most of De Waal’s work is characterized by this ‘historical’ view on social organization. To him, the animals’ social life is not only influenced by the selective pressures of the environment but also by the social history of the community. His book on the power struggle among male chimpanzees at Arnhem zoo essentially presented a historical narrative of social changes in one such community (De Waal 1982). His recent interpretation of bonobo behaviour was built on the idea that it developed through time as a means of reducing infanticide by males (De Waal and Lanting 1997). Finally, his most recent work dealt with the origin and dissemination of cultural patterns like handclasp grooming through time (De Waal and Seres 1997). Historical time is a key concept in much of De Waal’s evolutionary interpretations.

together specialists from different fields who discussed the relative merits of existing primate models, scrutinized the theoretical and methodological problems which primate modelling involved, and set out lines for future research. The volume, however, was not so much groundbreaking as a whole but elegantly demonstrated a historical transition which had taken place in the ideas on primate models. Some of the chapters continued in the time-honoured tradition of modelling based on single species: the book opened with Tanner's re-defence of the chimpanzee model and the gathering hypothesis, Susman proposed to learn more from bonobos and, rather originally, Crockett suggested that South-American howler monkeys might provide a fresh way of looking at hominids. It was the usual advocacy of certain species as 'better models', often based on personal preference or individual acquaintance with this or that animal. The fact that the chapters of the book's main section were more or less organized by genus (baboons, chimpanzees, howlers) testifies to this traditional approach to modelling.

Yet many contributors to the volume refused to be straitjacketed by this scheme and started to doubt the value of such models based on a single species (or a single genus). Quite independently from each other, people like Potts, Wrangham, Strum and Mitchell, Tooby and DeVore and even Susman and Crockett formulated critiques against this practice and came up with a number of alternatives. The pages of the same volume which presented some classical single-species models (most notably the one by Tanner) also contained a multidimensional attack against this long-standing method.⁴⁹

The weaknesses of referential modelling

The critics elaborated the hint which had come up in the wake of the bonobo debate, namely, that not just the bonobo, but any single extant primate poses problems if it is privileged as the best model for hominid evolution. Latimer et al. (1981: 476) and McHenry (1984: 218) had quoted Darwin's words from the *Descent of Man*: 'we must not fall into the error of supposing that the early progenitor of the whole Simian stock including man was identical with, or even closely resembled, any existing ape or monkey.' Susman (1987: 84), on his turn, made recourse to Haeckel's line: 'no single one of the existing man-like apes is among the direct ancestors of the human race'.

The most lasting contribution the Kinzey volume made was the distinction introduced by Tooby and DeVore between referential and conceptual models. In a referential model, they defined, 'one real phenomenon is used as a model for its referent, another real phenomenon that is less amenable to direct study'; conceptual models on the other hand were 'theories: sets of concepts or variables that are defined, and whose interrelationships are analytically specified' (Tooby and DeVore 1987: 184-5). Referential modelling was the method hitherto favoured in primatology, the whole suite from baboons to bonobos belonging to it.

49 It was at around the same time that Kortlandt (1986) condemned the simplistic assumption of homology of behaviour present in many chimpanzee models. He decried the lack of an intellectual tradition in primatology where the exigencies of fieldwork overshadowed in-depth analysis and extensive literature study.

Conceptual modelling of the sort comparable to Newtonian mechanics and Darwinian evolutionism still needed to be done: the study of human origins required a theoretical framework of causally interrelated variables which allowed new kinds of inferences to be made rather than using one phenomenon to uncritically ‘explain’ another. Tooby and DeVore started to develop the principles of such a framework, based on evolutionary and sociobiological theory which sees the genes as the unit of selection and animals as strategists promoting their inclusive fitness. This sociobiological approach had already been outlined by others (most notably Wilson), but Tooby and DeVore were the first to draw its implications for primate modelling. With conceptual modelling, Irven DeVore came full circle with his earlier work: once an originator of the first primate model based on socioecology, he now designed a way to move beyond this very practice by drawing upon sociobiology (Kinsey 1987: xv).

The distinction between referential and conceptual modelling was not totally unlike the one between formal and relational analogies which Alison Wylie (1985) introduced in the ethnoarchaeological debate at the same time. Despite their different ambitions (Tooby and DeVore desired to build a general theory of evolution, Wylie just wanted to improve the quality of ethnoarchaeological analogies), their basic concepts show considerable overlap. Referential models were like formal analogies which gave little consideration to notions of relevance, whereas conceptual models were like relational analogies that tried to work from causal connections between specific elements of the model towards testable inferences. ‘Referential modelling’ has since become the term to indicate all primate models we have encountered thus far and it was at these that the critics of the Kinsey volume aimed their arrows.

A first and often made point was that referential models might be all well and good but that they could not account for unique human characteristics. How could the baboon, the chimpanzee or the bonobo reveal anything significant about a species which had eventually developed language, bipedalism, complex tool-use and many other traits? ‘Only uniquenesses can explain uniqueness,’ Tooby and DeVore (1987: 187) argued, ‘one cannot invoke the features species have in common to explain their differences.’ Their statement echoed Jolly’s (1970: 9) warning that it was illogical to invoke the behaviour of a living species for explaining something that this species itself had not developed. There was a general sense that early hominid behaviour might have been very different from what we see among extant anthropoids. Assuming, like many previous modellers had done, that it was most likely similar to one single species in order to defend that species as a referential model is turning the whole issue around: we precisely want to learn what that behaviour was rather than assuming how it might have been! A priori assumptions about early hominid behaviour cannot be used in an argument to unveil that behaviour—this is committing the classical fallacy of the *petitio principii* or circular reasoning. Agnosticism about early hominid behaviour is often a better starting point than apriorism. Wrangham (1987: 51), therefore, found referential models restricted by their initial assumption:

they assume that the social organization of human ancestors was similar to that of a living species. Possibly it was. But it is much more likely that for several million years our ancestors have had forms of social organization not seen in species living today. This means that even “the best available model” (Tanner, 1981) may not be good enough.

Richard Potts, too, repeatedly pointed to this ‘problem of behavioral uniqueness’ (in line with his stone-cache model which was not based on primatological or ethnographic parallels as an alternative to Isaac’s !Kung-inspired home-base model). Providing the reader with a cautionary tale, he wondered whether we would be able to reconstruct the unique socioecology of hamadryas baboons on the basis of other extant baboons. The answer was of course no—which supported his view that unique features ‘may be masked by supposedly plausible reconstructions based on other primates’ (Potts 1987: 45). Even if early hominids are not conflated with chimpanzees, but placed along a chimp-human continuum, this still ‘precludes considering unique adaptations of that continuum’ (Potts 1987: 34). Such a ‘Piltdown approach’ to behavioural ecology (Tooby and DeVore 1987: 203) presents hominids as a mixture of living ape and human traits, ‘as a corridor, where chimpanzees enter at one end and modern hunter-gatherers exit at the other’ (Tooby and DeVore 1987: 203). In this sense, nothing has changed (apart from the species analogue) since Washburn suggested in the early sixties to see human evolution as the transition from a baboon to a modern hunter-gatherer.

Secondly, the critics agreed that there was no criterion for selecting a good source analogue. ‘The usual rationales for selecting modern primate analogues for the behavior of other primates, including early hominids, are not reliable,’ Potts wrote (1987: 45). Tooby and DeVore (1987: 186) named these models arbitrary ‘because there is no validated principle to govern the selection of an appropriate living species as a referential model’. Depending on the model one advocated, ecological analogy, subsistence analogy or phylogenetic homology were all put forward as the relevant criteria for making a choice. In the absence of consensus on such criteria, ‘analogies are often based on assumed or superficial resemblances’ (Potts 1987: 43). Similarity has become the sole yardstick, regardless of its genuine relevance. The argument devolves into some sort of ‘adaptive reasoning’ (Potts 1987: 34), i.e. reasoning which fits the source, but which is ultimately unverifiable. As a consequence, referential models come up with reconstructions which might seem possible but which never move beyond that stage. Possibility does not entail probability.

What the above critique came down to was the necessity to give consideration to the notion of relevance in modelling (and this is where the authors sided with Wylie). Tooby and DeVore were very explicit about this. Two species are always similar in some respects and different in others—no species is another—yet what is needed, according to them, is ‘a validated principle’ to determine what is important and what is not:

The question of which parts of the model are relevant (that is, display patterns of covariance) is usually not well specified, but left implicit, vague or intuitive. Consequently, there is no standard by which one can evaluate the large literature that discusses various species, and, at the preference of the author, asserts that hominids “probably” were like baboons, bonobos, or whatever, because they share some trait or other. (Tooby and DeVore 1987: 186)

Remark how they define relevance in terms of ‘patterns of covariance’, which is nothing else than causality within the source analogue. Rather than enumerating similarities, they wanted to see a distinction between relevant and irrelevant similarities, between causally connected aspects of the source and incidentally present aspects, between observed similarities and inferred similarities. They wanted to move from a *projection* of the source on the target to a *procedure* for moving between the two.

If there was no criterion to assess the relevance of similarity, there was certainly none to deal with dissimilarity. This ‘absence of any legitimate method for handling known differences between hominids and proposed referential model species’ had two ‘unfortunate effects’ (Tooby and DeVore 1987: 187): firstly, referential models could only be used for periods when the difference between source species and target species was thought to be minimal, i.e. the oldest periods; secondly, similarities were emphasized at the expense of differences. Since no distinction was made between relevant and irrelevant dissimilarities, all dissimilarity was seen as threatening the model and had therefore to be limited, either by minimizing the distance between source and target or by neglecting difference. The critique by Tooby and DeVore was implicitly directed at both chimp models: the chimpanzee model only worked for ancestral hominids, the bonobo model only for the last common ancestor and, as we have seen, they both over-stressed similarities at the expense of differences.

A third and more straightforward critique dealt with the poor concern for testing the primate model against the evidence from geology, archaeology and palaeoanthropology. For someone like the Palaeolithic archaeologist Potts, it was inconceivable to speculate on hominization without taking into account the archaeological evidence from the Plio-Pleistocene. Especially the problem of unique human adaptations could ‘only be resolved by the paleontological record’ (Susman 1987: 86). Referential models contained too much primatology and too little archaeology and geology. Although Tanner and Zihlman did incorporate a good deal of fossil and Palaeolithic evidence in their model, it is true that most referential modellers worked more on the source side of the analogy than on the target. Fruitful dialogue between primatologists and archaeologists or palaeoanthropologists was, and still is, rare, amounting to the impression that it is mostly primatologists talking to primatologists and palaeoanthropologists to palaeoanthropologists (Chapter 5).

Even if all the above problems could be coped with, referential modelling would still be theoretically problematic. This was the fourth critique. Strum and Mitchell (1987: 90) who were invited to the volume to write on baboons

questioned the very idea of a best model: ‘if all primates, in fact, all animals, can be understood in the same evolutionary terms, the concept of a “best” model must be faulty.’ A species’ behaviour was a highly complex adaptation to its environment, based on its own phylogenetic baggage, so that no one could simply serve as a model for another. The problems with referential models were not just of a practical sort but quite fundamental, which is reflected in the words of all critics: ‘there is no single primate species which serves as an appropriate socio-ecological analogue’ (Potts 1987: 47); ‘even “the best available model” (Tanner, 1981) may not be good enough’ (Wrangham 1987: 51); ‘the “model” I accept as the most likely representative of the earliest hominids by definition is a composite one’ (Susman 1987: 85); ‘simple analogies and direct comparisons between any one primate species and early hominids should be abandoned’ (Strum and Mitchell 1987: 103); ‘the use of the referential approach should be discarded’ (Tooby and DeVore 1987: 188); ‘paleoanthropologists must be prepared to discard prime mover and single-primate-species models of human evolution’ (Tooby and DeVore 1987: 236-7).

Reading through these severe objections, one can wonder if there was still any value in looking at extant primates at all for understanding early hominids. In fact, most authors believed there was. It served at least a truly heuristic purpose: rather than looking for definite answers, understanding living primates might at least lead to ‘productive questions’ (Wrangham 1987: 71) or ‘intelligent questions’ (Strum and Mitchell 1987: 95). It could also generate ‘testable hypotheses’ which had to be confronted with the archaeological record (Potts 1987: 43). Models should not be used for ‘projections’ but as ‘analytical tools’ (Dunbar 1989: 239). Heuristic value meant new questions, not an excuse for downright projections. In general, the view was shared that one needed to go beyond superficial similarities: ‘superficial similarities between early hominids and living primates [were] de-emphasized’ (Potts 1987: 47) in order to gain a genuine understanding of ‘processes and principles’ which ‘represent the only valid starting place for evolutionary reconstruction’ (Strum and Mitchell 1987: 103). Crockett (1987: 132) similarly refused to see howlers as models for hominids but sought ‘the application of larger principles of evolutionary theory’. Potts (1987: 44), in line with the Binfordian processual archaeology, said: ‘The emphasis is here on process, not static analogies.’ Processes, principles—it is remarkable how often these words appear in the critical chapters of the Kinzey volume. No matter how vague sometimes, it shows the desire to substitute an over-reliance on similarity between source and target to an understanding of the dynamic processes within the source itself. A fruitful use of a source analogue requires first an adequate understanding of that source itself, i.e. an understanding of the causal patterns between the elements of the source. There is a clear parallel here with Binford’s decision to go and study the Nunamiut (Chapter 3). Also frustrated by the ethnographic analogy of his time, he first wanted to understand the relationship between statics and dynamics in a present context (very much in line with the requirements of Wylie’s relational analogy). Understanding the internal dynamics of the source was to him a necessary prerequisite before the actual use of an analogy. It was necessary but not sufficient:

we have seen that the use of Nunamiut data in a Palaeolithic context remained problematic. The same holds true for Strum and Mitchell's understanding of the conditions that trigger hunting among baboons or Crockett's explanation of sexually selected infanticide or female emigration: these are excellent interpretations of primate behavioural patterns, but they do not tell us how to use them for reconstructing human social evolution. Whereas the horizontal relations of causality started to be elucidated, the vertical relations of attribute transfer remained to be investigated.

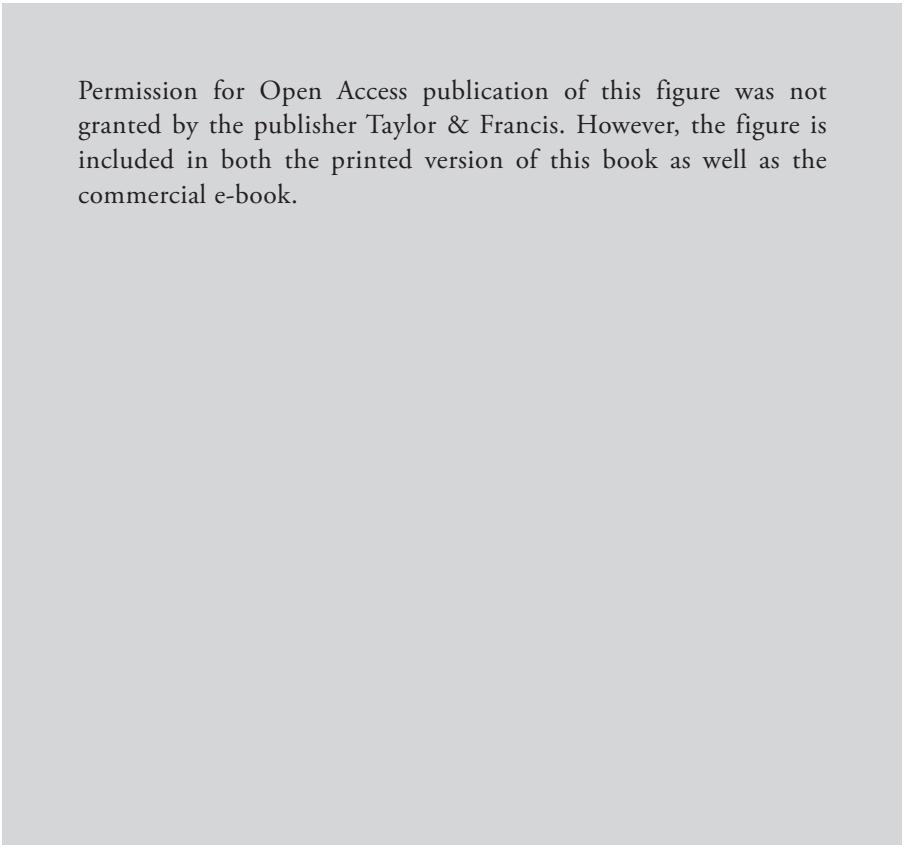
The Kinzey volume was a watershed in the history of primate modelling. While in many places reminiscent of traditional referential modelling (most notably in the structure of the volume itself), more than half of the authors raised objections against this way of working and sought alternative angles of research. It thus did to primatology what Binford had done to ethnoarchaeology and Boas to the comparative method: it called for relational understanding by observing a set of particular present phenomena, detecting relevant processes, isolating causes, and doing all this before any extrapolation towards the past is made. Apart from the justified but poorly elaborated claims in some articles to understand 'principles and processes', two chapters outlined directions which would become influential in the years following the book's publication. They were Wrangham's proposal for a phylogenetic comparison and Tooby and DeVore's outline of a conceptual model based on behavioural ecology.

Phylogenetic comparison or cladistics of behaviour

In his contribution to Kinzey's book, Richard Wrangham (1987) distinguished three ways of using great apes in human origins research: referential modelling, behavioural ecology and phylogenetic comparison. While the first was to be avoided, the second was much more interesting: behavioural ecology which attempted to explain animal behaviour in the present (by indicating the causal relationships between ecology and behaviour based on sociobiological principles as outlined by Tooby and DeVore) could eventually be used to predict fossil behaviours from a set of known variables. This was a promising but still tentative angle of research:

Despite the successes and growing importance of behavioral ecology, however, it is undoubtedly premature to rely on it to "predict" the behavior of fossil species [...] It may be only a few years before reliable explanations of species differences are found, but it is misleading to suggest that they have been found yet. The present state of the art means that attempts to use behavioral ecology to reconstruct hominid behavior are no stronger than those which use chimpanzees or other apes as models. (Wrangham 1987: 52)

The third option, phylogenetic comparison, offered 'a quicker, though ultimately weaker, system for recognizing probable aspects of ancestral social organization' (52). It consisted of comparing a list of behavioural patterns in the African apes (gorilla, chimpanzee and bonobo) and modern humans in order to find shared and derived character states (figure 20). If a pattern occurred in all four species, it probably had also



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Figure 20. Phylogenetic comparison maps behavioural traits onto the cladogram in order to determine which character states are ancestral, derived or unique. This one investigates the original primate social system, which was found to be female kin-based residence (Lee and Foley 1996: 56)

belonged to the common ancestor they once shared, since it is extremely unlikely that the same pattern would have been invented independently in the four lineages. This line of reasoning echoed Zuckerman's early claim that one had to look for commonalities between all the great apes. It also came close to Lubbock's quest for a common denominator between a variety of present source analogues. According to Wrangham, the aim was 'to distinguish between aspects of hominoid social organization which are shared, and therefore phylogenetically conservative, and those which are variable' (52). Shared primitive behaviours were ancestral to the common stock; shared derived behaviours had occurred later in a specific subgroup; and unique behaviours had evolved in only one particular species. Behaviours thus attributed to the last common ancestor were by implication also present at later hominid stages in human evolution as they belonged to an 'ancestral suite' (53).⁵⁰ After this theoretical defence, Wrangham compared

50 A very similar method was proposed by Reynolds (1966; 1968) in two isolated and largely forgotten articles published nearly twenty years before; they dealt with the evolutionary origin of open groups and kinship recognition.

the grouping patterns, the female, male, sexual and intergroup relationships of African apes and modern hunter-gatherers and found that the chimp-human common ancestor was ‘implied to have closed social networks, hostile and male-dominated intergroup relationships with stalk-and-attack interactions, female exogamy and no alliance bonds between females, and males having sexual relationships with more than one female’ (68). He stressed that this method was superior to referential modelling because it relied on several species, did not use them as models, allowed the common ancestor to have unique features and avoided criteria of unknown importance like hunting or savannah-dwelling. The weaknesses, he admitted, were that it considered only a limited repertoire of behaviours, neglected the importance of ecology and made no use of archaeological and palaeontological information.

Heavily drawing upon Wrangham’s work, Ghiglieri (1987; 1989) and Cameron (1993) presented emendations to his original phylogenetic comparison. Ghiglieri omitted gorillas from the comparison, keeping chimpanzees, bonobos and humans in it, so that more shared variables could be found for a more recent ancestor: not the common ancestor on the hominid-pongid split ca. 8 Myr ago, but the last common ancestor before the chimpanzee-human split ca. 4 Myr ago. As a consequence, his probable ancestral suite was expanded to incorporate a set of unique behaviours which had evolved in the chimpanzee-bonobo-human clade prior to splitting. The key change, according to Ghiglieri, was the emergence of male retention (also called male endogamy or male residence, the fact that male offspring remain in their natal social group while the females disperse to other groups). This, he suggested, led to cooperation between male kin in communal defence of the territory and communal reproductive strategies, ultimately resulting in a system of polygyny whereby individual males cooperated to enhance their reproductive success.⁵¹ Since male retention and communal breeding strategies which Ghiglieri observed among chimps, bonobos and even human societies ‘are so extremely rare among nonhuman primates [...] the chances that each of the most recent three species from the common ape-human stem evolved them independently seem extremely small’ (Ghiglieri 1987: 347). Ghiglieri went further than Wrangham by considering more variables for a smaller set of closely-related species. More importantly, rather than just enumerating shared behaviours, he also provided an explanation for the emergence of some unusual traits.⁵²

Cameron (1993) followed Wrangham’s procedure but cast it in cladistic terminology: phylogenetic stems were called ‘clades’, splitting events ‘cladogeneses’, ancestral characters ‘symplesiomorphies’, shared derived characters (characters present in several species but not in the ancestral stem) ‘synapomorphies’, and uniquely derived characters (only present in one species) ‘autapomorphies’. Though such cladistics of behaviour have grown in importance of late, Cameron’s recipe was not different from

51 One can think here of the alliances between dominant males in the Arnhem chimpanzee group which were indeed aimed at enhancing sexual access to females and, hence, reproductive success (De Waal 1982).

52 Following Tooby and DeVore (1987), Ghiglieri also attempted to formulate some explanatory principles of a conceptual model which moved beyond the mere descriptive conclusions of his phylogenetic comparison. Yet these ‘if, then’ propositions (dealing with such sociobiological themes like parental investment, kin selection and reciprocal altruism) were only loosely added at the end of his paper rather than forming a substantial part of it.

Wrangham's: take a number of species, compare them on a set of behavioural variables, and attribute shared traits to an ancestral stage and derived traits to more recent stages.⁵³ Cameron studied all great apes (gorillas and orangutan were included) and came up with character complexes for all common ancestors before the four major branching events (the orang split, the gorilla split, the human-chimpanzee split, the chimpanzee-bonobo split). Unlike Ghiglieri, he did not try to explain the development of new trends because this would 'merely result in the construction of an ad hoc model' (Cameron 1993: 405). What he did undertake, however, was evaluating archaeological theories on early hominids on the basis of the character complex he had defined for the last common ancestor before the chimpanzee-human split. Isaac's home-base theory, Binford's scavenging hypothesis and Potts' stone-cache model were compared with the results of his cladistic analysis of living primates. Scavenging was unlikely in his view because it implied that the incipient hunting in the last common ancestor should have been lost and reinvented afterwards. Stone-caching was only known from the chimpanzees in the Taï forest (Ivory Coast) which implies that it should have been lost by all other chimpanzee communities and bonobos.⁵⁴ Although he ultimately favoured Isaac's theory as 'the most likely behavioral scenario for the early hominids', this conclusion (which can be criticized for a number of reasons) is perhaps less important than the gratifying fact that primatological modelling and archaeological hypotheses were finally confronted.

Phylogenetic comparison (or 'cladistics of behaviour' to use the more recent term) is a straightforward method to gain an appreciation of the 'phylogenetic baggage' present in a common ancestor. It has the important advantage of not being confined to a single species. This explains why in recent years the method has become standard practice in much primatology: McGrew (1992: 40-64) used it to find out the origins of great ape tool use and the capacities for it in early hominids; Fruth and Hohmann (1996) did the same for nest-building competence (cf. Kappeler 1998 for a more elaborate assessment). However, there were certain important flaws to it. The method assumes that shared traits are by definition ancestral traits, apparently forgetting that similarity can also be caused by convergence. What holds for the wings of the bat and the fly may also hold for behaviour: comparable traits do not always need to be homologous but may be simply analogous.⁵⁵ Another disadvantage is that only conservative traits can be detected, i.e. traits shared by all species and therefore assumed to belong to the common ancestor. Innovative traits in the human lineage cannot be known by this approach, nor unique

53 Cladistic analysis results into a cladogram, i.e. the most parsimonious dendrogram needed to represent differences and similarities between taxa. Strictly speaking, such cladogram has no chronological dimension—it is just a visual rendition of the degree of affinity between species. Nevertheless, it is often believed to represent a phylogenetic tree—Cameron is no exception in making this assumption.

54 This critique is not entirely fair because Potts precisely tried to formulate an hypothesis quite independent from ethnographic and primatological parallels. Saying that it is unlikely because it is so unique does not do respect to Potts' interest for uniquely human features. Cladistic analysis allows to detect unique features but only if they have survived to the present day; unique features which have been lost in the meantime cannot be known and are said to be unlikely.

55 Phylogenetic comparison shares the same basic assumption as structuralist historiography, i.e. the seductive but unsubstantiated idea that similarity between occurrences implies underlying continuity.

characters which have not survived until the present day.⁵⁶ Phylogenetic comparison is supposed to tell what the situation was before hominization started, not how hominization eventually developed. A final flaw of the method was that its sole focus on phylogeny left ecology out of consideration. Behavioural ecology did the exact opposite.

Behavioural ecology

When Tooby and DeVore compiled their list of principles for conceptual modelling, one of the main sources of inspiration apart from sociobiology and evolutionary theory was behavioural ecology, the currently popular attempt to explain the uniquenesses of an animal's behaviour from ecological variables. At first sight this seems quite similar to the socioecology of the 1960s (cf. Crook and Gartlan 1966), but there are important differences. Firstly, whereas socioecology drew a one-to-one relationship between environmental context (understood as predator pressure and the availability and dispersion of food) and a type of social organization (Richard 1981), behavioural ecology is less rigid, less typological and prefers dynamic feedback systems with multiple variables over unilinear, causal relations (Dunbar 1989): 'there is no one-to-one correspondence between traits and selection pressures' (Tooby and DeVore 1987: 191). Secondly, whereas 1960s socioecology considered the group as the level at which selective pressures were most at work (simply because the group was taken as the unit of analysis, cf. Ghiglieri 1987: 322), behavioural ecology believes that 'selection acts at the level of the gene' (Tooby and DeVore 1987: 189).⁵⁷ Behavioural patterns are not explained in terms of the benefits they accord to the group but in terms of the individual's reproductive success and its genetic contribution to subsequent generations. If 'group survival' was the key to understanding primate behaviour in the socioecological paradigm, 'inclusive fitness' was the central notion for behavioural ecologists.⁵⁸ Inspired by game theory, individual animals were seen as strategic actors maximizing their own genetic potential in the given ecological and social context 'rather than inflexibly committed to the same behavior'

56 Of course, uniquely derived characters of the human lineage can be ascertained (they are simply the ones not shared by any other species) but Cameron rightly states that it would be tautological to use these to explain hominization, that is to explain their own emergence: 'The uniquely derived behavioral characters of extant human groups cannot be used to explain the behavior of the earliest hominids, just as an analogy based on contemporary chimpanzee populations cannot explain what the early Pliocene hominids were doing 4 million years ago' (Cameron 1993: 408). Human language, for instance, cannot be used to explain hominization. However, Dunbar (1997) compared grooming activities as related to group size and concluded that human speech emerged at the point that the size of communities went beyond a normal grooming capacity. Observing a close relationship between the size of the neo-cortex and the size of social groups among primates, he reasoned that speech emerged as a verbalized form of grooming in bigger groups, and that gossiping, i.e. the exchange of information on social affiliation, was its predominant function (as grooming is supposed to do among the great apes). Dunbar thus started from a shared phylogenetic basis (grooming) but by invoking the variables of group size and neo-cortex size he could account for a unique human feature (speech).

57 More recent forms of socioecology, however, avoid the pitfall of group selectionism (cf. Smuts et al. 1986).

58 Ghiglieri (1987: 322) phrases it very sharply: 'rather than being units of natural selection, social groups are ultimately the evolutionary consequences of individual adaptations, or strategies both to survive and to increase reproductive success by being social.'

(Tooby and DeVore 1987: 191). Tooby and DeVore developed the theoretical outline of such an approach, Wrangham saw it as promising but still in its infancy, but it was C. Owen Lovejoy who had already presented the first elaborate case of such reasoning.

In an often cited article published in *Science*, Lovejoy (1981) drew a picture of human evolution following the premises of behavioural ecology and sociobiology. At the outset, there was the basic assumption that evolution would strongly favour ‘any behavioural change that increases reproductive rate, survivorship or both’ (Lovejoy 1981: 344). He noted that among chimpanzees infant mortality was high due to the requirement of mother-infant mobility in subsistence activities. This mobility was also the principal restriction on birth spacing. If chimpanzee mothers would not need to travel with their young offspring, more infants would survive and births could be more frequent, so that survivorship and reproductive rate would increase. This was very much the ‘demographic dilemma’ (347) also faced by early hominids. Whereas chimpanzees stuck to their fragile solution, early hominids developed an alternative and ultimately more successful solution: females with offspring could stay at one place if they and their infants were provisioned by food-gathering males. For a male this would be a sensible thing to do if he was certain that the female’s offspring was also his: in that case, his efforts were no blind altruism but a way to promote the survival of his own genes. In other words, such form of male provisioning (or more in general, increased male parental investment) would only work if it evolved simultaneously with monogamous pair bonding between the sexual partners. This, Lovejoy held, was the behavioural base from which hominization started and he went on to argue that bipedalism, loss of sexual dimorphism, unique human epigamic features, and even the distribution of hominids over the world were the consequences, not the causes, of increased male parental investment and monogamous pair bonding.⁵⁹

Lovejoy’s evolutionary scenario received criticisms from several angles (cf. the discussion in *Science* 1982: 295–306). Wrangham with his reserved enthusiasm about behavioural ecology regretted that ‘even Lovejoy’s scheme, which is the most elaborate to date, is riddled with speculation’ (Wrangham 1987: 52). Feminists, not surprisingly, strongly disliked the minor role bestowed to women in this view of human evolution (Zihlman 1985; Fedigan 1986). Zihlman (1985: 374) named it a ‘used vehicle, propelled by outworn but retreaded notions’ which at one stroke ‘rehabilitated Man the Hunter but by co-opting Woman the Gatherer: now it’s Man the Gatherer, and Woman the Gene Receptacle is relegated to utter passivity.’ For our analysis, however, it is more important to see the limited attention given to phylogenetic issues in Lovejoy’s argumentation. Although chimpanzees function as a heuristic source, there is no consideration for the ancestral traits present in the early hominid. Lovejoy reasons from ecological conditions (food distribution, nature of food resources, required subsistence mobility) and life-history parameters (infant mortality, birth intervals, infant dependency and longevity). The particular nature of this ecological emphasis becomes clear when we contrast

59 Lovejoy was not the only one to work on such issues. Around the time of his influential article in *Science*, other sociobiologists discussed the evolutionary relationships of adaptations like paternal investment, concealed ovulation, menstrual synchrony and primate monogamy (Alexander and Noonan 1979; Benshoof and Thornhill 1979; Turke 1984).

Lovejoy's conclusions with the ones reached by Ghiglieri: whereas Ghiglieri's phylogenetic comparison had shown polygyny to be the ancestral trait, Lovejoy's behavioural ecology held that monogamy was the key feature for the same period. Male kin bonding with Ghiglieri had now become monogamous pair bonding with Lovejoy! This remarkable divergence was largely due to the methods used: phylogenetic comparison describes similarities between species; behavioural ecology explains differences between them. The first looks for conservative traits in the ancestral phylogeny of a species; the second for innovative traits induced by ecological and sociobiological factors. Typically, the first presents a list of shared traits whereas the second comes up with a narrative scenario. With conclusions so diametrically opposed, one wonders whether there was any notion of consensus in sight.

The most successful attempt to integrate behavioural ecology and phylogenetic comparison to date can be found in the work of Robert Foley and Phyllis Lee who held that 'while social states are partially constrained by phylogeny, they are also strongly influenced by immediate costs and benefits, and therefore can be analysed in terms of the principles of behavioural ecology' (Foley and Lee 1996: 54). Their 1989 *Science* article (but see Foley and Lee 1996 for a more extended argument) started with a list of previous models for the evolution of hominid social behaviour. Frustrated by this bewildering amount of conflicting interpretations, they provided an ingenious argument to limit the number of possible explanations. Firstly, they argued, the variety of social strategies is limited: if you see that the males and the females of any species have four basic social options (being solitary, associating with kin, with non-kin or with extended kin), the combination of both shows that no more than sixteen social strategies can be possible. Even admitting that each of these strategies can be stable or transitory does not bring the number above 32 potential options. (The 1996 paper leaves out the category of extended kin which reduces the number to 18). Secondly, not only the number of social states is finite, the number of viable evolutionary pathways from one state to another also is. A social system based on stable alliances between non-related males (as with the baboons) cannot evolve into a totally different chimpanzee-like sociality with kin-related males and nonkin-related females without losing evolutionary fitness. Thirdly, constraints on the evolutionary pathway are not only determined by the distance between two social systems, but also by the ecological context in which the evolving species lives. Social evolution is thus limited by a finite number of options, by phylogenetic constraints on the possible evolutionary pathways and by ecological constraints in which evolution takes place. Foley and Lee repeatedly stressed that phylogeny and ecology complemented each other:

any evolutionary change is the outcome of the interaction between novel selective pressures and the existing structure of behaviour. Evolutionary change is therefore not just a product of a new environment, but of how the existing structures interact with the new environment. (1996: 53)

To understand human sociality, they started with a cladistic phylogeny of Old World primates in order to identify ‘the most probable social characteristics at the various branching points that have occurred in the evolution of the Hominoidea’ (1996: 55; cf. figure 20). This showed that male residence (combined with female dispersal) and male kin-bonding was a typical innovation in the African ape/hominid clade (contra Ghiglieri who believed that this pattern only evolved in the chimpanzee/human clade). Though this conclusion was ultimately based on living species, Foley and Lee stressed that ‘it is not individual species that are being used to determine the ancestral state (in the form of an analogue model), but the overall pattern of variability’ (1996: 57). They then went on to study how these ancestral states interacted with the selective conditions posed by new environments. Their interpretation of the earliest hominid social structure as consisting of ‘mixed sex groups, with males linked by a network of kinship’ eventually built ‘the selective pressures of open tropical environments onto the social state of the evolving African hominoids’ (1989: 904).

Though their eventual interpretations are not always original nor entirely devoid of speculation, the importance of Foley and Lee’s work lies first and foremost in the combination of a phylogenetic approach with an interest in behavioural ecology. They thus escaped several of the criticisms from the Kinzey volume: they explicitly refused to draw upon single-species models but used a phylogenetic comparison instead, they sought for explanatory principles based on behavioural ecology and life-history theory, and they accounted for unique patterns in the hominid clade. Although this model made considerable use of fossil evidence and palaeoecology, the concern for the archaeological record was rather poor—a flaw also present in Wrangham’s and Tooby and DeVore’s work. In this sense, the final critique of the Kinzey volume was still left open for correction. Ethoarchaeology was an attempt to solve this problem.

Ethoarchaeology

A former student of the late Glynn Isaac, Jeanne Sept (1992) questioned whether the accumulations of stone and bone débris on Plio-Pleistocene sites in East-Africa were really reflections of early hominid home-base activities. Isaac’s home-base theory had led to much controversy (with criticism coming from Binford and notably Isaac himself), but the way Sept approached it was rather original: by looking at chimpanzees. Isaac had devoted his life-work to the study of early land use patterns, but he was wary of the use of external sources, believing that the archaeological and palaeoanthropological record still constituted ‘the most powerful clue we have to the beginnings of the human evolution’ (1981: 152). Nevertheless, he suggested that certain accumulations of material might misleadingly look like home-bases. This would have been the case, for instance, with repeated individual feeding in the shade of long-lived tree. Sept decided to work out this hypothesis. She studied chimpanzee nesting sites along the Ishasha River in Congo (then Zaire) and noted that over the course of several years certain trees were re-used as favourite sleeping places (figure 21). Although these trees did not function as fixed home bases, their re-use resulted in an accumulation of physical

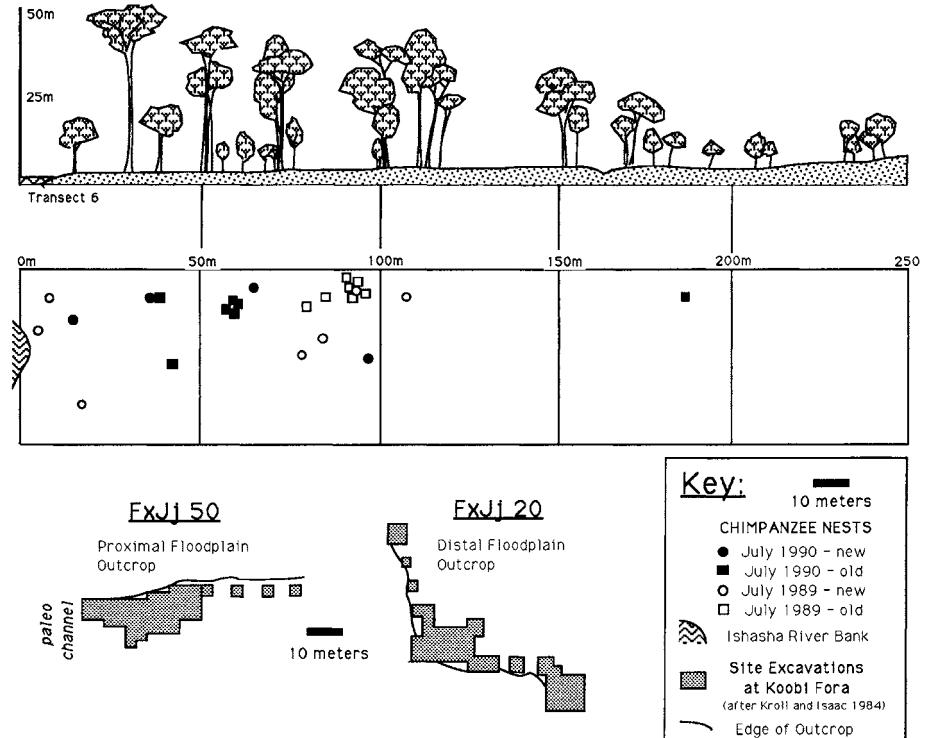


Figure 21. Ethoarchaeology as undertaken by Sept has a cautionary function. It shows how certain trees in a chimpanzee territory may be revisited over the years as nests, thus resulting in an accumulation of débris without ever being used as homebases (Sept 1992: figure 4)

residues like nests, feeding debris and faeces. Rather than permanently occupied locales in a central-place foraging economy, these trees were like ‘Hilton hotels’ which attracted ephemeral residents and produced accumulations of material residue. Sept’s analysis did not mean to tell ‘that early hominids had chimpanzee-like ranging or subsistence behavior’ (196) but that activities very different from home-base behaviour could produce a similar home base-like material record. In the end, her cautionary tale focused on the problem of equifinality, or the problem that the same result might have been caused by a very different process (Sept 1998). The question why chimpanzees re-used certain places was, however, left open. Sept just suggested that it might have to do with fruit density and canopy height, but also inter- and intraspecific food competition, territoriality, predation risk and mental mapping.

At the very heart, Sept’s work was an actualistic or middle-range research which tried to see the formation processes between causal dynamics and material statics at work in a present-day context. She undertook ethological fieldwork with an explicitly archaeological research question in mind; a motivation very comparable to the ethnographic turn many archaeologists took in the late 1960s. Her ‘ethoarchaeology’ was therefore the simian variant of ethnoarchaeology, it was heavily indebted to the Binfordian discussion on actualistic research (Chapter 3) and has

been flanked by some other field studies since (Joulian 1994; 1996). In a more recent paper, Sept has explicitly drawn the parallel between this sort of ethoarchaeology and the actualistic field studies undertaken by ethnoarchaeologists. She noted that while primatologists have not documented the spatial and material correlates of tool use and nesting with the appropriate detail necessary for ethoarchaeological analysis, archaeologists, on the other hand, have long neglected great ape behaviour and have only recently ‘scratched the surface of the rich repertoire of artifact use’ (Sept 1998: 89).

Though Sept’s source analogue consisted only of chimpanzees (in contrast to the many species present in a phylogenetic comparison), she, too, believed that ‘one should not use chimpanzees as simple analogs or “referential models” [...] for early hominid behavior [...] any more than one should study living hunter-gatherers as mere vestiges of prehistoric times’ (196). This remark betrays Sept’s background in archaeology and her familiarity with the ethnoarchaeological discussion on the proper use of analogy. In the discussion which followed her article in *Current Anthropology*, she referred to this discussion and especially to Wylie’s distinction between formal and relational analogy to explain her disavowal of referential modelling. Sept does not look for a ‘best model’ based on the amount of similarity but seeks ‘organizational relationships among sets of variables relevant to the formulation of models for prehistoric situations’ (Hutterer, quoted by Sept 1992: 196). Since relevance is considered, not all differences between source and target are threatening the analogy. Again drawing a parallel with ethnoarchaeology, she states:

In the same way that ethnoarchaeological studies of modern humans who use metal cooking pots and weapons can be used to develop models of the activity patterning at Stone Age archaeological sites [...], studying the spatial patterns of chimpanzee activities that produce debris, durable or ephemeral, can help archaeologists systematically explore alternatives to the existing behavioral models of early site formation. (190)

Sept’s article marks an interesting turn because for the first time the long-standing archaeological discussion on analogy was integrated with the long-standing history of primate modelling.⁶⁰ That the substantial part of the paper was only based on a limited data set gathered during no more than three months of fieldwork does not detract from the paper’s methodological merits of confronting data on extant nonhuman primates with a specific archaeological research problem while taking into account the problems of analogical reasoning. (The fact that few weeks of observations could already lead to interesting results is to be seen as promising rather than problematic; cf. Sept 1998.) In this sense, Sept’s ethoarchaeology replied to the last criticism of the Kinzey volume which decried the lack of concern for archaeological and palaeoanthropological evidence among primate modellers.

60 Interestingly, at about the same time Robert Foley (1992) also discussed primate and hunter-gatherer analogies in the same logical terms in his contribution to the *Cambridge Encyclopedia of Human Evolution*.

The ongoing lure of referential models

In the *Current Anthropology* discussion which flanked Sept's article some scholars took issue with her rejection of referential models. McGrew (in Sept 1992: 198) admitted that they had their drawbacks but there was 'one big advantage': 'Living organisms can provide us with both behaviour and artefacts, while concepts supply neither.' Similarly, Moore (in Sept 1992: 199) noted that 'nothing is gained by labelling a model "conceptual" or "referential" [...] the distinction that Tooby and DeVore draw is artificial and misleading.' Commenting upon Sept in a later issue of *Current Anthropology*, Quiatt and Huffman 'agreed strongly' (1993: 69) with Moore, McGrew and others in their defence of referential models. Apart from leading to a number of promising, new research directions, the Kinzey volume, and particularly the article by Tooby and DeVore, apparently also met with serious opposition from authors who still defended the use of referential modelling.

The earliest and most elaborate criticism came from an article by Stanford and Allen, also published in *Current Anthropology*, who found that the distinction between conceptual and referential models had 'more academic than scientific utility' (1991: 58). In their view, conceptual models never really escaped a reliance on extant species:

The currently proposed "conceptual" models (Foley and Lee 1989; Tooby and DeVore 1987) are in fact chimpanzee-referent models couched in Darwinian terms to give the appearance of a broader evolutionary perspective and therefore greater empirical strength. (Stanford and Allen 1991: 59)

In the end, conceptual models only resulted in interpretations which 'are essentially those of DeVore and Washburn (1963) and Washburn and Lancaster (1968)' (59); they did nothing else than resurrecting Man the Hunter from its ashes. This was a curious objection: even if the existing conceptual models seemed to resemble Man the Hunter-like interpretations, this was not a necessary corollary of their being conceptual and even if they seemed to draw upon 'an implicit chimpanzee analogy', this was not what the authors did (quite the opposite!). Moreover, the fact that the results of conceptual modelling happened to resemble older scenarios or chimpanzee-like models, cannot be an argument to dispel the method altogether. Methods should be evaluated on their internal logic, not on whether we like the results they come up with. Haraway's evaluation of the gathering hypothesis which said that we 'cannot proceed only by writing the tales one wants to be true' (1984: 90), applies here too.

How such a renewed sort of referential modelling should be envisaged is suggested by a more recent article by Stanford (1996).⁶¹ Interested in the emergence and importance of meat-eating in the hominid diet, Stanford turns to chimps who also hunt, eat meat and share it on a regular basis. Though he carefully delineates the behavioural ecology of chimpanzee meat eating and hunting, the application of his insights to the hominid realm does not differ from traditional referential modelling based on a straightforward

61 Recently, Stanford has published these views in a popular volume (Stanford 1999) where phrasing may sharper, but reasoning more shallow.

projection from the source onto the target on the basis of some immediate or assumed similarity. The mere presence of a particular trait among chimpanzees was sufficient reason to attribute it to the hominids. To Stanford, hominids were assumed to hunt, to cooperate in this activity and to share the obtained food ‘given the systematic occurrence of all three of these behaviors in chimpanzees (and therefore their likelihood in earliest hominids)’ (1996: 96). The reverse was also true: the absence of the required trait in other extant primates dispelled them from the study. Symptomatic in this respect was Stanford’s exclusion of bonobos simply because ‘they appear to hunt less frequently than do chimpanzees’ (1996: 97). Referential modelling of this sort devolves into an exercise in circular reasoning: we study chimpanzees because we assume hunting to be important in human evolution, and because chimpanzees hunt we say that early hominids must have been doing this as well. This was the same sort of circularity social evolutionists like Tylor had been guilty of. They too had assumed something to be primitive because it looked primitive. Such reasoning consists of a classical *petitio principii* whereby that what should be proven is used as an argument of proof.⁶² Even if Stanford’s paper concluded with a list of ‘hypotheses to be tested’ and some ‘fundamental differences between the meat eating of modern chimpanzees and that of evolving Pliocene hominids’ (108), the preceding self-fulfilling prophecy could no longer be undone. Surely, if you want to learn about the origin and structure of early hominid hunting, it cannot suffice to study one species which happens to practice hunting and to neglect another phylogenetically equally close species which does not. The low frequency of hunting among bonobos is as important as its high frequency among chimpanzees.

Moore and McGrew took more subtle standpoints in that they saw the potential of conceptual modelling but warned that this was ‘a young, developing and still uncertain field’ (Moore 1996: 285). In anticipation of more mature applications, referential models remained a valuable source of information: they suggested important new ideas and helped to formulate hypotheses about early hominid behaviour which could be tested independently.⁶³ ‘The main advantage of a referential model,’ writes McGrew (1992: 199), ‘is its concreteness, especially from the viewpoint of empirical testing.’ Moore (1996: 278) added: ‘It can generate a detailed scenario from which testable predictions can be derived.’ Testing was what justified the further use of referential models (although it was a step which both McGrew and Moore never took). This reliance on testability will also be found with the early New Archaeologists who believed that it did not matter where the hypothesis came from (analogy, empathy, or fantasy) as long as it was independently verified in the archaeological record. Even Moore who presented the most subtle defence of referential models—he even mentioned the discussion on analogy in logic and archaeology—never made the move from testable to tested hypothesis. This is unfortunate because it would have made clear many of the difficulties such testing programme ran into. In archaeology, Hodder and Wylie argued that such tests rarely

62 In logical terms we can say that the *explanandum* is used as its own *explanans*.

63 Because of their reliance on similarity, neo-referential models, like the earlier ones, were also strictly limited to the period of the pongid-hominid transition. In archaeology this was not the case. A prehistorian like Steve Mithen (1994) could easily invoke nonhuman primates to talk about *Homo erectus* in Britain since immediate similarity was not believed to be critical.

amount to more than indicating further similarities between source and target. A truly independent and conclusive test is never within reach.

It is not easy to understand the ongoing popularity of referential modelling after the heavy critiques and the viable alternatives formulated in the late 1980s. Part of the difficulty lies in the fact that it is such a recent phenomenon, making its importance difficult to assess. However, certain factors can be proposed even if they are rather externalist ones. Firstly, it is noticeable that most of the recent advocates (McGrew, Moore, Stanford) are scholars with extensive fieldwork experience with chimpanzees; indeed, all ‘neo-referential models’ are in fact chimpanzee models.⁶⁴ It is not unlikely that the strong acquaintance with the animal which has so long been favoured as the best model prevents these authors from looking at other species (McGrew, pers. comm.), let alone accepting recent claims that such models should be abandoned altogether. Secondly, all ‘referential’ defenders come from a background in North-American primatology (McGrew, Moore, Stanford, Allen, Quiatt, Huffman), whereas many proponents of alternatives either come from outside North-America (Wrangham, Foley, Lee, Dunbar) or outside primatology (Tooby, Lovejoy, Potts, Sept).⁶⁵ An even more remote outsider like Richard Dawkins, for instance, could happily note: ‘The very idea of taking animals to be role models, as in the bestiaries, is a piece of bad poetic science. Animals are not there to be role models, they are there to survive and reproduce’ (1998: 211). The tenacity of referential modelling reflects perhaps the special nature of American primatology which, unlike its European pendant, started as a branch within anthropology with the explicit incentive to learn about human evolution especially by constructing models (Fedigan and Strum 1997). If this is true, today’s discussion on the pros and cons of referential models would go back to the very origins of primate modelling nearly half a century ago.

Beyond single-species models

The discourse on primate models in the last decade has been much more branched than ever before. Rather than being characterized by the orthodoxy of one single-species model (like baboons in the 1960s and chimpanzees in the 1970s), diversity of opinion is large and many issues remain unsettled. The profound doubts against referential modelling, already ushered in the critiques on the bonobo model, were a phase of fundamental reflection from which several alternatives sprouted. A Kuhnian terminology could apply here: after the failure of the bonobo model, the period of normal science which followed an accepted procedure like referential modelling was interrupted for fundamental, theoretical discussion; this did not immediately lead to a novel paradigm but to a much more diversified,

64 The term ‘neo-referential’ is justified to set these recent chimp models apart from the single-species models that came before. Apart from McGrew, all advocates were new to the debate, they all reacted against Tooby and DeVore, and they differed profoundly from the earlier chimp models. Tanner and Zihlman used chimpanzees to distress the importance of hunting, Stanford used them to emphasize it.

65 Wrangham (though now working in Harvard), Dunbar, Foley and Lee belong to the British school of ethology; Tooby is an evolutionary psychologist, Lovejoy a biological anthropologist, Potts and Sept are archaeologists.

polyparadigmatic period (phylogenetic comparison, behavioural ecology, ethoarchaeology) where even defences of the old paradigm, i.e. referential modelling, were still regularly voiced. In the absence of a generally accepted new paradigm (or in the transition towards it), a variety of theories, methods, and relevant data sets burgeoned.

In logical terms, the criticism expressed in the Kinzey volume repeatedly decried the lack of concern for relevance in the assessment of similarity between source and target. Similarities were said to be superficial or assumed; there was no way of handling observed differences; there was no way of evaluating the different primate models available. Critics asked for processes and principles, not static similarities, which would allow to make human uniquenesses visible rather than obscuring them by wholesale projections.

Building on that critique, phylogenetic comparison was an attempt to strengthen the analogy by expanding the source: both the number and the variety of species under consideration were enhanced. To recapture the words by which Foley and Lee defended their phylogenetic comparison: ‘it is not individual species that are being used to determine the ancestral state (in the form of an analogue model), but the overall pattern of variability’ (1996: 57). By allowing a greater number and a greater diversity of species in the source analogue, substantial patterning was sought instead of random similarity. The fact that species in the source and target were phylogenetically related only eased the attribution of characters from the one to the other.

Behavioural ecologists improved the analogical arguments by a different means, namely by studying the causal connections within the source. If phylogenetic comparison described the existence of a correlation on the source side, behavioural ecology attempted to account for it. It focused on the vertical relation of causality in the hope of being able to predict social behaviour in the past from a number of given variables—an ambition which has been rarely put convincingly into practice. Even if a source was understood in terms of causality, the uniformitarian assumption that the same causes were also at work in the past was hard to prove. Nevertheless, behavioural ecologists stressed, like Wylie had done before, that it was impossible to use a source in an analogy or model without prior understanding of its internal causal mechanism.

Sept’s ethoarchaeology also worked on the notions of causality and uniformitarianism. By showing that very different behavioural processes could produce similar material results, she provided a cautionary tale on the complex relationships between statics and dynamics. Though her source was restricted to only one species, her interest was in the causal principles governing the formation of the archaeological record.

Testing was the strengthening device suggested by those who continued to work with referential models. Needless to repeat, testing is not a means to improve the analogy but to verify an analogical inference. Testing focuses on the truth of the conclusion, not on the validity of the argument. As such, it is replete with all the difficulties outlined in Chapter 1.

Expanding the number of source contexts, enhancing the variety of source contexts, establishing causal links in order to assess the relevance of similarities, testing analogical inferences: the discourse on primate modelling in the last decade reads like the analogy chapter in a textbook on inductive logic. Different ways of reinforcing analogies were systematically explored now that the amount of similarity was no longer the sole criterion for choosing models. Maurice Dorolle wrote it already in 1949: '*Le nombre des ressemblances ne suffit donc pas*' (149).

Conclusions

Whenever studying an episode in the history of science, there is always the pitfall of characterizing it through its most extreme statements—very often the ones later critiques and textbooks have reified into the received caricature younger generations of researchers grow up with. The preceding sections, however, have endeavoured to nuance such schematic picture of the history of primatology. Rather than lumping together this variability in the convenient form of a final conclusion, it might be more useful to go through the six analogical strength criteria, as we have done previously, in order to reveal the differences.

The strength of primate models

In terms of *number of sources* summoned, nearly all primate models, from the earliest baboon comparisons to the most recent chimp models, were confined to a single species: the gelada, the wolf, the common chimpanzee, the bonobo—even the baboon model, though referring to an entire genus, was largely based on studies of the single species of olive baboons (*Papio anubis*). Chimpanzees were by far the most popular source of inspiration: apart from their ubiquitous role in the feminist evolutionary scenario, they figured in many other reconstructions, in the few ethoarchaeological case studies, in Lovejoy's behavioural ecology of early hominids and in all neo-referential models. Only two approaches diverged from this pattern and strengthened their analogy by increasing the number of sources: the social carnivore model, particularly the proposals by Schaller and Lowther (1969) and King (1975; 1976; 1980) who argued that the inclusion of carnivores greatly expanded the source analogue, and the phylogenetic comparisons or cladistics of behaviour (Wrangham 1987; Ghiglieri 1987; Cameron 1993) which looked for shared traits across a great number of primate taxa.

This predilection to focus on a single primate species is somewhat surprising. Of course, 'experts on ape behavior like to claim that their subjects are the only or best model of the last common ancestor' (De Waal in Stanford 1998: 407) but the consequence of such 'my-own-species syndrome' can be grave. Hominization was a process that took place over several millions of years, a process which by definition implied change and variation and which involved many species. Today, there are hardly 200 extant primate species to model this varied process. Primate variability is thus limited

to a relatively small sample that is even rapidly disappearing. The decision to focus on a single species is, therefore, simply pernicious.⁶⁶

As an obvious consequence, the *variety of source contexts* was also largest in these two approaches. Phylogenetic comparisons enhanced the variety by looking at all African great apes rather than a single species. A more recent study on the origins of nesting behaviour even applied the method to all living primate species, including the prosimians (Kappeler 1998). Apart from the bonobo and the chimp, the galago and the aye aye are now included in the source. Social carnivore modellers, too, worked from a varied source base: they included animals as diverse as the wolf, the hyena and the lion and they even made reference to nonhuman primates and contemporary hunter-gatherers. Clearly, the strength of causal patterns observed in the source is greatly increased if it holds across a variety of source contexts. It is surprising, therefore, that most primate models have stuck to source contexts as homogeneous as possible.

Increasing the *number of similarities* between source and target is an often invoked strategy for improving analogies. This, however, was not always done. The earliest baboon studies, for example, worked first and foremost from observed differences between baboons and humans, since these could indicate the trajectory of human evolution from its primate substrate. Models based on social carnivores and geladas, too, did not seek to enhance the amount of observed similarity in a strictly quantitative sense but rather focused on a single trait of supposedly relevant resemblance (in subsistence ecology or dental morphology). It was only with the chimpanzee and bonobo model that the degree of overall similarity became the dominant criterion, even to the extent that a form of quasi-identity was sought in order to allow wholesale projections from source to target. All sorts of similarity (behavioural, biochemical, morphological, ecological, etc.) were thus summoned. During the subsequent crisis of referential modelling, the critics reacted precisely upon this over-reliance on the quantity of similarity instead of its quality. Phylogenetic comparison, behavioural ecology and ethoarchaeology were all attempts to move beyond the criterion of numerical resemblance in order to find some relevant patterning—a tendency now reversed by the most recent defences of referential modelling.

A similar pattern is tangible in the way different modellers dealt with the thorny issue of *dissimilarity*. Here too, it seems that the models based on baboons, geladas and carnivores as well as the more recent critical alternatives could happily cope with dissimilarity by distinguishing between ‘structure-violating’ or ‘structure-preserving’ differences (Holland et al. 1986: 299), whereas the chimpanzee and bonobo modellers, on the other hand, considered every form of dissimilarity between source and target as ultimately weakening the argument. As long as the amount of similarity is the sole validating criterion, clearly, any difference is seen as threatening. The very opposite was true for the social carnivore model of Schaller and Lowther (1969): the difference they noted between nocturnal hunting

66 Lockard was right when he said: ‘There are too few ape species, each too specialized, for easy use of the comparative method’ (Lockard 1971: 177).

among carnivores and diurnal hunting among humans did not weaken but strengthened the argument—early hominids could fill in an ecological niche of daytime hunting, a period during which social carnivores were nonactive. Authors who picked up the criticism of the Kinzey volume agreed that differences were as instructive as similarities: ‘That leaves us with the alternative of trying to learn what we can by examining comparatively both similarities and differences in the behaviors of modern primates’ (Schubert 1991: 8). In any case, it was clear that ‘differences between two species cannot be explained by taking one of them as a model for their last common ancestor’ (Cartmill 1990: 189).

Though the chimpanzee and bonobo models mostly relied on the criterion of similarity, it would be erroneous to assume that they did not consider issues of *relevance*. Of course, they did—but it was another definition of relevance than with the previous models. The ‘mature’ baboon model (as it appeared in the 1963 paper by DeVore and Washburn and by all later textbooks and popular accounts) and the social carnivore model were both based on an ecological analogy: social behaviour was the result of environmental parameters, understood in terms of eating or being eaten (food availability versus predator stress). Important differences notwithstanding—according to the carnivore model, early hominids had been predators but according to the baboon model, they had been prey animals—there was a shared conviction of a one-to-one ratio between behaviour and ecology. Indeed, knowing the subsistence and the environment was in fact enough to predict the social organization.

The reverse was true with the chimp and bonobo model which replaced ecology for phylogeny as the relevant criterion. The belief that there was any single correspondence between behaviour and ecology had been seriously undermined now that primate variability started to be better understood: transfer of behavioural attributes could no longer be executed on the basis of a handful of ecological variables. Instead, the stunning degrees of genetic affinity between chimps, bonobos and humans which biochemists had discovered, made it much more useful to look at these phylogenetically close allies. Not a shared environment, but a shared set of genes lay at the base of these models, and hence an overall similarity in anatomy and behaviour. After that, the tension between an ecological and phylogenetic understanding permeated into the subsequent crisis of referential modelling: behavioural ecology stressed the ecological component whereas cladistics of behaviour was more turned towards the phylogenetic component (the work by Foley and Lee (1989) being a rare attempt to reconcile both). Relevance was certainly an issue at stake throughout the entire debate on primate modelling, though it increasingly left out the discussion on causality in the source. After the optimism of the ecological modellers who had come up with an unambiguous and uniformitarian causality, there was less and less confidence that such simple correlations really existed. Enumerating likenesses became more popular than substantiating their relevance.

Finally, how did primate models balance the *weight* of the conclusion against the weight of the premises? Apart from a few exceptions, the answer is: badly. Nearly all models were far too ambitious in their inferences of the past on the

basis of what they had. This is not just true of the projective models who wanted to give a visualized image of the past (like the mature baboon model, the chimpanzee model and the bonobo model), but also of the more argumentative forms like most carnivore models and recent alternatives. They all attempted to give an encompassing view of the key characteristics of social life at a certain stage of human evolution, often flanked by an evolutionary scenario of how early hominids moved on from that stage onwards. That is a lot of interpretation to rest upon a terrestrial habitat, a hunting subsistence, a convergence in DNA, or female immobility to name but a few of the premises. There are only two exceptions to this pattern. Jolly's gelada model was more humble in that it only wanted to deduce early hominid diet from a present source (although he did not refrain himself from inventing yet another origin story from that base). And Sept's chimpanzee parallel simply pointed out that very different behavioural dynamics might have been responsible for the formation of the so-called living floors than the ones hitherto assumed. Overall, however, the conclusions derived from the modelling activity greatly outweighed the premises of available similarity and causality.

Looking at the way primate models were defended and strengthened, the history of debate can be recapitulated as follows. In a first episode, which roughly ran from the early sixties to the mid-seventies, baboons, social carnivores and geladas were used to draw inferences about the past. The approach consisted of analogical reasoning from fairly distant sources (imported analogues), on the basis of few observed similarities which were made relevant through considerations of ecological or subsistence analogy, dissimilarities in this respect not necessarily being structure-violating. A second episode, from the mid-seventies to the mid-eighties, witnessed the emergence and popularity of referential models based on both chimpanzee species as a reaction to the textbook version of the baboon model. Here, the approach consisted of projective modelling from close sources (manifest analogues), on the basis of numerous similarities required by the premise of phylogenetic homology. Dissimilarity was much more problematic and seen as structure-violating. Whereas in the first episode, extant primate species were used for a partial transfer of attributes to early hominids, in the second episode (but already foreshadowed by the final variants of the baboon model) these species turned into wholesale projections onto the past. It is also in this context that the metaphor of visualization first occurs: among some well chosen living primates, one could 'see', 'get a picture' or 'find an image' of early hominid life. From the mid-eighties onwards, such extreme forms of referential modelling have been criticized and alternatives were sought, although chimpanzees are still being favoured as 'the best model available'.

A change in approach

What explains this marked shift in referential modelling around 1975? Why did similarity become so important after that? Several causes seem to have been at work. Firstly, it has already been noted that the increased knowledge on behavioural variability between and within primate species, often living in similar environments, discredited a straightforward ecological approach and that the

discovery of close biomolecular affinity between humans and both chimpanzee species favoured a phylogenetic perspective. Since in the ecological paradigm (see in particular Crook and Gartlan 1966) a strong, unambiguous and uniformitarian causal connection between environment and behaviour was postulated, a minimal amount of observed ecological similarity was enough to justify the transfer of attributes from source to target. Greatly increasing the amount of similarity did not add to the argument. Under the phylogenetic paradigm, on the other hand, scholars looked among extant primates for the closest approximation of a fossil species, or what they called ‘the best model’. But since two species are never exactly the same (otherwise they would have been just one species), it became a matter of arguing that they were quasi-identical. The consequence was that asserting the amount of similarity became the required form of argumentation in the phylogenetic paradigm. Due to the changing knowledge on primate behaviour and genetics, earlier referential models focused on the vertical relation of causality, while the later referential models (the ones based on both chimpanzee species) worked on the horizontal relation of similarity.

There is, however, a second reason for this shift. Whereas the early referential models (pre-1975) sought to draw inferences about aspects of the past, later referential models (post-1975) were predominantly burdened with the task of finding a good model. Emphasis shifted from the target to the source; no longer the left side, but the right side of the analogical scheme became problematic. Whereas earlier scholars had been more interested in the mapping and transfer phases of the analogical algorithm, later referential modellers were concerned with the retrieval phase. In this context, it is important to recall the psychological experiment where students were asked about military intervention in an identical war situation whose details recalled World War II in one case and Vietnam in the other (Chapter 1). The replies (positive in the first case, negative in the second) very elegantly demonstrated the importance of surface similarity during source selection; it was only when mapping and transfer phases come into play that more structural similarity was considered. Of course, there is quite a difference between American undergraduate students individually responding to a questionnaire after some minutes of reflection and the methods used in a long-standing discussion in the history of primatology. Nevertheless, it cannot be accidental that surface similarity played such important role at a time when the quest for a best model (retrieval phase) stood high on the agenda and, inversely, that structure similarity was more essential at times of inferential mapping and attribute transfer as with the early referential models like the gelada model.

These insights from cognitive psychology might help to clarify what happened around 1975—a shift from the mapping and transfer phases to the retrieval phase, and a concomitant shift from structural to surface similarity—but they do not yet explain *why* this happened.

So, thirdly, we need to place the change in primate modelling against the broader context of shifting theories about animal behaviour. The earlier referential models emerged in the context of socioecology, the later ones in the context of sociobiology. Socioecology sees the environment as the key determinant to

behaviour, sociobiology thinks behaviour is driven through genetic calculations. The first focuses on food (on eating or being eaten), the second on sex (on copulating or not). The first takes the group as the unit of analysis, the second the gene. The first thinks in terms of group survival, the other in terms of individual reproductive success. Suffice it to compare the titles of two representative books: Kummer's synthetic monograph *Primate Societies* was subtitled *Group Techniques of Ecological Adaptation* (1971); De Waal's study of male dominance and reproductive strategies in the Arnhem chimpanzee colony appeared under the title: *Chimpanzee Politics: Power and Sex among Apes* (1982). Thus, the genetic turn in biology did not only supply primatologists with evidence for a very close affinity between the great apes and humans, but also provided a new and powerful theory for understanding behaviour—sociobiology. Just like Washburn and DeVore, Schaller and Lowther and the textbook writers were attuned to Crook and Gartlan's paradigm of adaptive relationships between social organization and environment, so did Tanner and Zihlman drew upon sociobiological principles such as Trivers' notion of parental investment and sexual selection.

It was against the background of these shifting paradigms that a change in observation methods also occurred. Whereas baboon scholars during the 1960s had been watching troops of animals as a whole (often through binoculars or from the roof of the jeep), in the early 1970s individual recognition of each animal through long-term habituation, with or without artificial provisioning, became the new standard (Carpenter had been the first to do this, Goodall made it popular through the personal names she gave to her chimps, J. Altmann (1974) wrote a classical outline of such individual sampling methods—the paper is still the best-quoted publication in primatology to date). So even in terms of data gathering, the emphasis shifted from group behaviour to individual behaviour. In the ecological paradigm, basic environmental similarity was enough to predict the social system. In the sociobiological paradigm, it became necessary to stress phylogenetic affinity, anatomical resemblance and behavioural similarity to argue that the same selective processes operative among chimpanzees were also present with early hominids.

Primate modelling, primatology and archaeology

The debate on primate modelling has considerably altered during the last half a century, and so has its place in the rest of primatology. Once the *raison d'être* of the young science called primatology, it has now evolved into one among the many items on the discipline's research agenda. It is perhaps no longer the most respected approach, but the promise that studying primates might elucidate aspects of human origins is still firmly rooted in the discipline's self-justification. When Hooton coined the term primatology in the mid-fifties, it was to indicate a research field which dealt with the study of primate behaviour in the wild with the explicit anthropological purpose of clarifying human evolution. Washburn implemented that anthropological suggestion, but his student DeVore rejected the ancillary role assigned to primatology. For the 1960s generation of baboon experts

(the Altmanns, Kummer, Rowell, Hall and DeVore himself), the interest was in living baboons, not in fossil bones. Their role in the popularization of the baboon model was therefore rather minimal.

During the two following decades the pattern remained mostly the same. Primatologists documented primate behaviour in the wild but the modelling business was often left to scholars from other disciplines. Though chimpanzee workers like Reynolds, Kortlandt, Goodall and Teleki showed a decided interest in issues of human evolution, the most vociferous proponents of a chimpanzee model came from physical and feminist anthropology. Dunbar was perhaps a rare exception of a practising primatologist who turned his field data on geladas into an argument on hominid evolution. In general, however, most mainstream primatology of these years was not concerned with primate modelling, no matter how often field workers superficially claimed that their species was ‘the best model’. Livingstone named the mountain gorilla ‘the most plausible ancestor’ (1962: 301); Kummer considered the hamadryas baboon’s one-male unit society ‘a better model of human social structure than that of the chimps’ (1971: 152); MacKinnon found the loosely-organized community system of the Sumatran orang-utans ‘the best common model’ (1978: 147); McGrew, Baldwin and Tutin saw the open-country living chimpanzees of Mt. Assirik as ‘the best available model for inferring the processes of adaptation in our ancestors’ (1981: 241). These brief statements show how field workers often suffer from ordinary ‘species patriotism’, i.e. the belief in the superiority of the primate they know best to reveal human origins—a practice not restricted to nonhuman primates only if we may believe Adriaan Kortlandt (1986: 126):

I can recall reading such statements referring to animals ranging from apes and baboons to wolves and dogs, to rats and elephants, to seals and dolphins, to geese, gulls and cormorants, and finally to bees, ants and termites.

This species patriotism, or what Kortlandt named ‘palaeoanthropomorphism’, is understandable because ‘it is a promising argument for fund-raising and publicity’ (Kortlandt 1986: 126) and because it reveals the disproportionately large acquaintance field workers have with their ‘own species’.

The mid-eighties crisis in referential modelling was steered by some primatologists like Strum, Wrangham and DeVore but mostly by scholars with different disciplinary backgrounds: Tooby was an evolutionary psychologist, Potts, Cameron and Sept were archaeologists, Lovejoy, Foley and Lee biological anthropologists. The neo-referential reaction against this criticism came exclusively from American chimpanzee experts but their emphatic defences of a chimpanzee model all appeared in anthropological journals like *Current Anthropology* or *American Anthropologist*. Nowadays, primate modelling is no longer the alpha and omega of the discipline of primatology but a contentious zone of discussion between several research traditions.

It is telling of the debate’s sequestered nature that none of the models here discussed appeared in one of the four leading primatological journals—*International Journal of Primatology*, *American Journal of Primatology*, *Folia Primatologica*, and *Primates*—though

human evolution was explicitly mentioned as an area of concern in the editorial statements of at least two of them.⁶⁷ Part of this absence is due to the fact that the *IJP* and *AJP* only started to appear around 1980, precisely at the time when referential modelling became discredited. Whereas *Folia* and *Primates* were both established around 1960, they belonged perhaps too much to their Continental and Japanese ethological traditions to be interested in such American invention like modelling. Another reason is that the natural science format of all these journals (particularly their requirement of statistical proof) could badly accommodate the more speculative evolutionary scenarios based on extant primate species. As a consequence, defences of referential models appeared in other publications. These were very often conference proceedings (all Washburn's papers were published in these, but think also of the more recent the Kinzey volume), less often monographs (Tanner 1981) but mostly non-primateological periodicals like the feminist journal *Signs* or one of the major anthropological journals (particularly *Man* in the early seventies and *Current Anthropology* in the last decade). *The Journal of Human Evolution* functioned as an important forum until the late eighties when editorial policy shifted towards a more stringent natural science approach. *Nature* and *Science* published three articles which caused much debate (Zihlman et al. 1978; Lovejoy 1981; Foley and Lee 1989) but none of it was printed in the great primateological journals. These publishing politics epitomize how primatology had disengaged itself from its anthropological roots and how primate modelling, once the incentive to that discipline, was turned into a relatively independent area of discussion.

Does the fact that most articles dealing with primate models now appear in major anthropological journals indicate a genuine dialogue of primatologists with palaeoanthropologists and Palaeolithic archaeologists? The answer is negative. 'Genuine interdisciplinary collaboration,' McGrew wrote in his discussion of Sept's paper, 'between primatology, especially of the African apes, and palaeoanthropology, especially in archaeology and ecology, is long overdue' (in Sept 1992: 197). Part of the absence of such debate stems from an implicit disagreement as to what constitutes the primary evidence for reconstructing human evolution. Archaeologists and palaeoanthropologists (like Potts, and White and Johanson who co-authored the Latimer article) stressed the primacy of the fossil record, whereas primatologists believe that living primate systems provide a better starting point. This causes a lot of mutual misunderstanding: primatologists are often poorly familiar with the fossil and archaeological record (e.g. in the discussions on primate tool use), whereas archaeological treatments of primatological findings are sometimes depressingly impressionistic (e.g. Mithen 1994).

Despite some recent attempts at bridging (cf. Sept 1994; 1998; Runciman, Maynard Smith and Dunbar 1996; Steele and Shennan 1996), the promise of genuine cross-fertilization of ideas seems further than ever now that archaeologists have displaced the origin discourse to the emergence of anatomically modern humans in the Middle Pleistocene. While chimpanzee and bonobo experts reach further and further into the past, well beyond the hominid-pongid split

67 But see the theme issue on primate nesting and resting in a recent volume of *American Journal of Primatology* (1998, 46, 1) which contained the papers by Kappeler and Sept.

at 5 Myr, Palaeolithic archaeologists have centred the discussion on human origins to a much more recent date. The clash between the multi-regional hypothesis and the replacement model, the largest palaeoanthropological and Palaeolithic debate of the last decades, centred on that period. The 1960s quest for 'the origin of man' has now been substituted by a broad interest in 'the emergence of modern humans' and the interest in australopithecines has been replaced by a fascination with Neanderthals. Since the existing primate models are strongly geared towards elucidating older periods, and since most archaeologists are badly aware of the primatological discussion, a fecund exchange of ideas is probably not to be realized soon. This is certainly unfortunate considering the archaeological experience with analogical reasoning and the richness of relevant primatological observations primatologists. With this remark, however, we have already embarked on a comparison of disciplines.

A comparative history of debates

The three preceding chapters each dealt with the history of a particular debate which could be read in its own terms. However, if we want to gain a balanced view of the use of ethnographic and primatological parallels, we will have to confront these debates with each other. This is what the present chapter sets out to undertake. The appropriate way to do so is by means of a triangulation. First the comparative method will be juxtaposed to ethnoarchaeological analogy, then ethnoarchaeological analogy to primate modelling, and finally primate modelling to the comparative method. In each case, similarities and differences between debates will be highlighted and interpreted. Drawing a comparison between two debates follows to a considerable extent the procedure of drawing an analogy between a source and a target: it also starts from observing similarities and differences, it assesses the relevance of each of these and tries to account for them. Whereas the development of ethnographic analogy between the nineteenth and twentieth centuries has already been dealt with to some extent, much more attention will be put in the juxtaposition of primatology with each of these.

The comparative method and ethnoarchaeology

The use of analogical comparisons in the twentieth-century ethnoarchaeology differed profoundly from its nineteenth-century forebears. It has been sufficiently argued that, from the 1970s until the rise of postprocessual uses of ethnography, the criterion of similarity lost its importance in favour of the consideration of causality. Consequently, the point was no longer one of finding nearly exact analogues that could be projected holistically onto the past, but of finding relevant processes that could be extrapolated. There was, as one of the contemporaries phrased it, ‘the need to compare *processes* rather than frozen-packaged societies suspended in an eternal “ethnographic present”’ (Spriggs 1977: 13, original italics).

Projections and processes

It is remarkable how strongly *processual* authors (the name is not incidental), from the earliest days to the latest attainments, expressed their disavowal regarding the projective approach. In his famous introduction to *New Perspectives in Archaeology*, Binford wrote: ‘We assert that our knowledge of the past is more than a projection of our ethnographic understanding’ (1968b: 90). Even if Binford was at that stage still trustful of testing, the use of projections was already out of the question. The 1970s witnessed the enormous expansion of ethnoarchaeological fieldwork.

Writing about such inspiring fieldwork, Watson nonetheless warned: ‘No matter how strong this “You are there!” feeling becomes, however, it is essential to resist the temptation to make wholesale transfers from the ethnographic to the archaeological’ (1979: 278). Even more abstract models which resulted from decades of fieldwork could still not be earmarked as stand-ins for the past. Whitelaw, for instance, took care that his refined inferences were ‘not simply a projection of the same simple model onto the past’ (1991: 183). From testing over field-work to model-building, a strong suspicion towards wholesale projections was present in all stages of processual archaeology. The reason for this reluctance was the importance attached to the individual history of every contemporary society. From Gordon Childe through John Yellen to Ian Hodder, nearly all twentieth-century archaeologists accepted the point which the Duke of Argyll and Franz Boas had been at great pains to demonstrate to their evolutionist contemporaries, i.e. the idea that all present cultures had always had a historical trajectory of their own, regardless of how backward and therefore how representative of the remote European past they initially seemed. Bryony Orme paraphrased this general attitude which prevailed in ethnoarchaeology very well when she wrote: ‘For present-day primitive societies are *not* a recapitulation — they are not a colour supplement 3D reconstruction of the development of the embryo that became western civilization’ (1973: 490).

Modern ethnoarchaeological analogies, therefore, diverged on nearly all logical strength criteria from the Victorian socioevolutionist parallels: the *number* and the *variety* of sources were greater, *similarities* and *dissimilarities* were weighed in terms of *relevance*, and the *weight* of the premises vis-à-vis the conclusions was properly balanced. The reason for this comprehensive and manifest discrepancy is simply historical: all archaeologists who had raked up the use of ethnographic parallels in the twentieth century distanced themselves from the nineteenth-century comparative method. After the heyday of historical particularism in anthropology and the culture-historical approach in prehistory, no one could simply pick up the thread and continue as if nothing had happened. No one could, and few really wanted. Indeed, the excesses of Sollas’ thinking, whose reprinted work was still widely read and heavily criticized, had cast a gloomy shadow on all forms of ethnographic comparison. Returning to present-day non-industrialized societies thus required a careful positioning at a safe distance of the comparative method. Grahame Clark was particularly critical of Sollas’ approach which was ‘not only unfashionable; it was also overdaring’ (1951: 54). Earlier, Clark had already expounded the ‘grave error’ committed by the evolutionists whose comparative ethnography had been derived, and unrightfully so, from comparative anatomy (1939: 171). Childe, on the other hand, was evidently appreciative of Victorian scholars like Morgan and Engels, but he shunned their use of ethnographic projections and of what he called ‘the “shreds and patches” theory of culture’ which only reasoned from superficial similarities (1946: 250). Crawford was perhaps the only one who sought to continue the Victorian tradition, but in practice his mere juxtaposition of prehistoric and ethnographic material evidence was far removed from the classical-evolutionist projective approach as practised by Morgan, Tylor and Sollas but only faintly echoed the early Lubbock or Evans.

It is true, users of the ethnographic analogy in the first half of the twentieth century also heavily relied on *similarity*, but the definition of that similarity was radically different. It entailed historical continuity, spatial proximity and functional equivalence in ecology and economy. To the Victorian scholars these were rather irrelevant parameters compared to the degree of technological complexity and the degree of civilization in general. On top of that, the *weight* of the conclusions differed profoundly: Clark's and Crawford's piecemeal transfers of functional and technological information were crumbles compared to the wholesale social, legal, and cultural projections of Tylor, Morgan and Sollas. Archaeologists escaped the comparative method not so much by a radically different logic, but by a much more prudent application of the same logic that was now based on sound similarity and well-balanced transfers.

This sceptical attitude filtered through in the 1960s. Despite the rise of a neo-evolutionist anthropology, archaeologists were wary to circumvent the old comparative method. Even Ascher whose new analogy was somehow reminiscent of the socioevolutionist tradition literally said that he was 'anxious to avoid the mistakes of the early evolutionary school' (1961: 319). And in the *Man the Hunter* volume, J.D. Clark spoke of 'the initial disastrous essays of the late 19th century' (1968: 280). It would be totally erroneous, as has often been done, to interpret the New Archaeology's interest for ethnography as a revival of nineteenth-century evolutionism. In anthropology neo-evolutionists like Marvin Harris c.s. sought to align themselves with the comparative method, but in archaeology this was simply not the case. In fact, the further development of ethnoarchaeology in subsequent decades must be seen as an elaborate answer to the shortcomings of the old comparative method. The same holds true for the contextual archaeology which Ian Hodder defended by stating that, unlike the Victorian evolutionists, it 'did not depend on finding "primitive" societies' (1982a: 40).

Clearly, then, the uses of ethnographic analogies in the twentieth century differed from the comparative method simply because scholars explicitly opposed themselves to the latter. Through textbooks and undergraduate courses, most scholars in Anglo-American archaeology were acquainted with the legacy of socioevolutionist thinking, or at least with a simplified version of it. The work of Lubbock, Tylor, Pitt-Rivers, Morgan and Sollas was often mentioned in such general introductions to the discipline, if only to show how *not* to reason. Even a more subtle work like Trigger's *History of Archaeological Thought* (1989: 145-7) remained quite critical of this tradition. On top of that, the racist undertones in the evolutionist writings, or at least the undertones which after the Second World War started to be regarded as racist, did not particularly contribute to a continued popularity. The comparative method has not only been over-studied by historians of science, up until this day it is also schematically known, though poorly appreciated, by practising archaeologists. If ethnoarchaeology is distinct from the comparative method on most of its principles, it is because it was defined in opposition to this laden legacy. The difference between both debates can simply be accounted for by the historical development of archaeological debate in the nineteenth and twentieth centuries. Unfortunately, the answer is less straightforward for the other axes of our triangulation.

Ethnoarchaeology and primate modelling

Though the discussion on the proper use of ethnoarchaeology and the quest for an adequate primatological model coincided in the second half of the twentieth century, the differences between them became sometimes surprisingly large. Comparing primatological and archaeological journals from the 1970s and 80s, one is surprised that the arguments on analogy were roughly coeval. In more recent years, as Chapter 4 showed, there has been some convergence between both fields, particularly in a subdiscipline like ethoarchaeology. Initially, however, there had also been a hint of possible parallelism between the two debates.

The impact of functionalism

Looking at DeVore and Washburn's defence of the baboon model and putting it side to side with Clark's use of an Inuit analogy for Star Carr, several convergences can be noted. Apart from the obvious but not always relevant differences—East-Africa versus north-west Europe; savannah baboons versus subarctic hunter-gatherers; Plio-Pleistocene hominids versus the Mesolithic—both shared a structural emphasis on subsistence as the key to understanding other aspects of the communities studied, subsistence being defined as the food procurement through the economical exploitation of the surrounding ecology. For DeVore and Washburn, food distribution and predator avoidance were the key variables which determined the baboons' social organization. For Clark, subsistence literally was 'the economic basis', to rephrase the title of his best known monograph, for any further inquiry. 'Subsistence', he had once written, was 'the most vital aspect of the life of prehistoric, or indeed of any communities' which influenced 'every aspect of the life of the community' (1939: 177). Consequently, when external sources were invoked, both DeVore and Washburn and Clarke defended them by pointing out similarities in economy and ecology. The baboon model was defended because Australopithecines had had a terrestrial lifestyle and subsistence structure in a savannah habitat. The Inuit analogy was drawn because the Mesolithic people at Star Carr had also been hunter-gatherers in a relatively comparable setting. In both cases, then, analogies were drawn on the basis of a single similarity that was deemed relevant.

The reason for this convergence must be sought in the theoretical reservoir both had tapped. It has already been mentioned how the early students of baboon social life were inspired by functionalist anthropology. Washburn had declared that Malinowski's functional theory worked better for monkeys than for humans, whereas DeVore was influenced by Radcliffe-Brown's theory of society (Gilmore 1981). Functionalism which regarded society as an integrated whole where the constituent elements all contributed to the maintenance of the social order was thus influential to the first generation of primatologists. In archaeology, Clark too was attracted to this form of anthropology. He named the functionalist view of human society which resulted from 'intimate field studies, like those carried out by Radcliffe Brown, Malinowski, and their pupils and followers [...] full of promise to the prehistorian whose evidence is necessarily vestigial' (1939: 174). Such

analyses showed that societies ‘are integrated wholes; that the various elements in their cultures are interrelated; and that indeed, they acquire their meaning for the societies concerned by the way in which they are organized’ (174). With its integrative, holistic view of society, functionalism—much like the later systems theory—vaguely promised the ability to reconstruct badly documented parts of the social system on the basis of other well-preserved parts. However, in practice both DeVore and Clark turned to some form of ecological causation whereby the given environment determined or heavily constrained the social outcome. Such external causation was in fact the very opposite of what Radcliffe-Brown had defended.¹ But even in their specific reading of his work, both Clark’s analogy and DeVore and Washburn’s model converged into a form of ecological determinism.

However, it should not be forgotten that Clark’s Inuit analogy was really an exception seen within the broader context of his work where direct-historical analogies were much more frequent and extensive than this few lines of general-comparative reasoning. On top of that, functionalism was not generally embraced by Clark’s contemporaries. Childe, for example, severely criticized its lack of historical attention, which was to him, as a historical materialist, quite unacceptable (Childe 1946: 247–8). Clark’s analogy drawn from contemporary Eskimos was, therefore, an isolated instance before the rise of processual archaeology and this is why Ascher heralded it as a rare example of the new analogy. The overwhelming majority of archaeological analogies formulated on both sides of the Atlantic during the 1950s, 60s and early 70s were simply direct-historical arguments which tried to force the analogy of resemblance into a homology of descent. And in this they were diametrically opposed to ecological analogies then furthered by the primate modellers.

Archaeologists and primate models

If ones compares the overall tendencies in both these postwar debates, they appear to be going in opposite directions. The discussion on ethological models started from a preference of ecological and subsistence analogies (the baboon model, the carnivore model, the gelada analogy) which turned into a predilection for phylogenetic homologies (the chimpanzee model, the bonobo model). The use of ethnographic sources in archaeology, on the other hand, initially favoured a homologous approach (the direct-historical approach of the functionalist, Americanist and early New archaeologists) but eventually developed into a strictly analogical treatment (the general-comparative approach, the mature processual ethnoarchaeology and the early contextual ethnoarchaeology). In terms of argumentative logic, primatology moved from causality to similarity, archaeology from similarity to causality. In both cases the breakpoint laid around 1975: this was the moment when the first chimp models replaced the previous baboon, gelada and carnivore

1 Against Malinowski’s theory of needs which explained cultural expressions as the satisfaction of external physical, physiological or psychological needs, Radcliffe-Brown had stipulated, following Durkheim, that social facts were to be explained by social facts. Even if they had once originated as answers to external needs, social phenomena had to be understood in terms of present meaning and purpose—hence, the largely a-historical character of functionalism (Kuper 1996: 48–9).

analogies; it was too the moment when ethnoarchaeology broke away from the argument of historical continuity, the unpromising testing of hypotheses and the wearisome cautionary tales. Overall, however, both debates had developed autonomously and largely independently from each other and, what's more, in radically opposite directions.

To appreciate the discrepancy between the debates, it is well worth to consult and to contrast some of their respective spokespersons. At a time when primatologists were already vehemently quibbling over the best possible model for understanding the behaviour of Australopithecines, the archaeologist David Clarke poignantly observed:

It would not only be poor science but it would be a scientific tragedy if we forced the social and economic life of Australopithecus into the recent pattern of either so-called modern 'Primitives' or the highly evolved pattern of the modern apes.
(Clarke 1972b: 41)

This is a crucial quote: what to most primate modellers was considered a laudable and a necessary enterprise, was nothing short of a scientific tragedy to one of the key thinkers of the New Archaeology! Students of animal behaviour were assessing the relative merits of the baboon model, the social carnivore model or the gelada analogy, but Clarke found such claims downright outrageous even well before the holistic projections from chimpanzees and bonobos were made. There is perhaps not a single locus in the history of the debates which better illustrates the discrepancy that had grown between archaeology and primatology. Clarke was not the only archaeologist holding such ideas. In fact, his essay was widely read and became influential; the ideas expressed in it were by no means isolated. Take for instance Murray and Walker who, sixteen years after Clarke, equally decried the quest for a best model: 'Scientific methodology has less to do with choosing and testing a single elegant model,' they wrote, 'than it has to do with choosing analogies in terms of interesting properties which behave in consistent ways in polythetic sets of commensurable cases' (1988: 263). The idea that some sources in the present would provide a privileged access to the past was ridiculed by many contemporary practising archaeologists.

The disapproval of referential modelling expressed by archaeologists went hand in hand with a critique on the amount of formal similarity as the decisive criterion for assessing the analogy. 'Goodness of fit is not enough,' David Clarke (1972b: 41) wrote when evaluating analogical inferences. This statement perfectly echoed the logician Dorolle's already oft-repeated but compelling dictum that '*le nombre des ressemblances ne suffit donc pas*' (1949: 149), but was totally at odds with the contemporary arguments in primate modelling where similarity became all-decisive. Whereas several primatologists were in the midst of committing the fallacy of the perfect analogy in terms of chimp and bonobo models, Murray and Walker persistently stressed that 'an analogy is not an equivalence' (1988: 275): 'Such phrases as "a window on the past" carry connotations of a two-dimensional image, picturing reality, so to speak. A better analogy is that of listening to echoes from the past. We have no score, all we know is that they represent transforma-

tional processes of human impingements' (277). However, it is important to realize that this criticism was not directly aimed at the advocates of referential models in primatology, nor were Clarke's commentaries. These archaeologists mainly spoke to fellow archaeologists. There was not only a discrepancy between both disciplines but also a tacit discretion. The gap between both debates yawned, and it did so in silence.

Primateologists and ethnographic models

The opinions primatologists held about the use of ethnographic comparisons also testifies to the extent of divergence between ethnoarchaeology and primate modelling. On repeated occasions, primatologists turned to the evidence from contemporary non-industrialized societies, if only to counterbalance their input from the non-human primates. From Washburn and DeVore's (1961: 102) juxtaposition of baboons to 'preagricultural humans' to McGrew's (1992: 131) comparison of Tasmanian aborigines and Tanzanian chimps, primate modellers have often relied on ethnographic evidence to explain the process of hominization. The early hominid was approached by a referential sandwich, consisting of an ape bun on the one side and a forager bun on the other. Quintessential to such 'Piltdown approach' (Tooby and DeVore 1987: 203) to evolutionary reconstruction was not only that the best-fitting primate species be found, but also the best-fitting hunter-gatherer society. By 'best-fitting' was generally meant the 'lowest', though that word was rarely used explicitly in the second half of the twentieth century. Reluctant to echo old racist undertones and discriminatory discourses, several primatologists nonetheless presented substantial arguments for highlighting this baseline of human adaptation in the present.

Typically, such arguments started by a formulaic apology for singling out specific societies as closer to early hominids or pongids but stressed the necessity to do so. Linda Marie Fedigan wrote that in the absence of a proper debate on analogy it was 'nevertheless necessary here to accept the usage [of analogy] and to go on to the question of which, if any, of the modern hunting and gathering societies provide the most appropriate analogies' (1986: 45). Bill McGrew realized how 'social and cultural anthropologists might think such a comparison to be a waste of time, believing the gap between human and non-human culture to be so wide as to be unbridgeable' but stressed: 'The basic point is this: *We will never know if such comparisons are useful unless we try them*' (1992: 122, original italics). And Gordon Hewes (1994: 62) stated: 'Comparing even the least complex human society with that of chimpanzees is likely to offend some defenders of the notion that all cultures are exactly equal by whatever standards we may apply.' Yet he continued: 'I should make it very clear that none of the "backward" human cultural groups that I have mentioned lack the potential (now increasingly realized) for rapid entry into modern complex societies' (62). The fact that "backward" groups could be rapidly Westernized apparently stressed their humanity, but did not prevent Hewes from considering them as evolutionary relicts.

Whereas the terms ‘lowest’ and ‘highest’ were avoided, the underlying concepts were retained and labelled in terms of relative degree of ‘complexity’. McGrew restricted it to the safe realm of technology: ‘One should seek the most complex technology in the non-human species, and the simplest one from the human array’ (1992: 134). Hewes, on his turn, used complexity in the widest possible sense of the word: ‘We must find one or more human groups that lie at the absolute minimum of complexity yet manage to survive without direct or indirect outside support’ (1994: 60). There is *some* irony in this preference for the word ‘complexity’. In an attempt to avoid rigid cultural hierarchies, primatologists fell back upon this ostensibly more neutral term, a term, however, which had been the key concept of Spencerian evolutionism. Unaware of this historical linkage and totally independent from the processual move away of nineteenth-century evolutionism in archaeology, Fedigan, McGrew and Hewes were thus deeply involved with a search for the ‘least complex human society’.

Typical in their quest was a reliance on an external authority to decide upon the matter. Not being acquainted with the field of hunter-gatherer studies themselves, they all invoked an anthropological expert to guide their exploration. ‘The most systematic attempt to answer this question is by the French ethnographer Alain Testart,’ L.M. Fedigan (1986: 45) wrote. W.C. McGrew (1992: 131) needed ‘a comprehensive but precise, rich yet objective taxonomy that is neither ethno- nor anthropocentric’ and found that ‘the most apt typological system is that of W.H. Oswalt’. Likewise, Hewes (1994: 61) relied upon Murdock’s ‘impressive worldwide ethnographic database’, i.e. ‘the admirably organized and indexed Human Relation Area Files (HRAF) archives.’ Of course, there is nothing wrong with drawing inspiration from key thinkers of a related discipline, especially when one is, for all sorts of practical reasons, unfamiliar with that specific field. Yet what was rather detrimental to this enterprise was the exclusive trust bestowed upon each of these and the concomitant uncritical acceptance of their ideas. Testart’s ‘most systematic attempt’ was after all an only nine-page long article in the popular journal *Pour la Science* (1978). Oswalt’s ‘objective taxonomy’, useful as it is, posed serious difficulties to draw a scale of technological complexity purely on morphological grounds.² And Murdock’s HRAF archives, although heavily criticized in recent years, provide mere descriptions of peoples; the files do not tell which tribe was the most primitive or least complex of all. Again, relying on external experts is inevitable if one wants to undertake genuine cross-disciplinary research, but singling out one author as the key authority for settling a complicated debate inevitably leads to distortion and simplification. The eagerness of some primatolo-

2 Oswalt’s system counts the ‘technounits’ of basic tool types as an average measure of technological complexity. A spade, for example, contains at least a handle, an iron blade and a device for conjoining the two, resulting in three such technounits, which is a higher form of complexity than the single technounit of a simple digging stick. The problem with this categorization is that it only looks at the formal end product of a technological process. According to it, a boomerang would be as complex as a chopper or a digging stick, both containing only one technounit. This totally ignores the complexity of both tool production (the manufacture of the boomerang) and consumption (the throwing of the boomerang). Oswalt’s system is useful to describe tools, not to devise technological hierarchies (cf. Mithen 1996: 74-6).

gists to find the one and only anthropological expert reflects the same avidity by which they sought the single best primate model.

Following Testart's 'careful and well-reasoned analysis', Fedigan (1986: 45) found 'the !Kung and Australian Aborigines [...] to have structural features that would make them good choices as models for earlier foraging societies,' whereby she had a personal predilection for the !Kung. McGrew (1992: 134), on his turn, preferred the Tasmanians because they were 'said to have had the simplest technology of all human foraging peoples.' He admitted that another tribe like the Australian Tiwi could equally suffice, considering their equally simple subsistence technology. Hewes (1994: 61), however, disagreed with this choice: 'I have therefore chosen the cultures of Mrabri of northern Thailand and the Tasaday of Mindanao in the Philippines to compare to the chimpanzees, retaining the aboriginal Tasmanians already used in McGrew's study chiefly to emphasize the far greater simplicity of Mrabri and Tasaday culture'. Far greater simplicity, that was at stake. 'Often the San (Bushmen) and the now extinct aboriginal Tasmanians are represented as the "most primitive" and hence most like early humans. I propose two other groups as better representatives for comparison with chimpanzees' (Hewes 1994: 59). The cultures thus described 'can provide information that contributes directly to our understanding of the behavior and behavioral capacities of modern human beings, the earliest hominids, and existing chimpanzees' (Hewes 1994: 61). The !Kung, the Tasmanians, the Mrabri or the ill-reputed Tasaday, primatologists thus debated, selectively guided by anthropologists, where on earth the least complex human culture could be found. Whereas David Clarke regarded such endeavour as 'a scientific tragedy', several primatologists consented to Hewes' remarkable urge that 'we must find one or more human groups that lie at the absolute minimum of complexity' (1994: 60).

Needless to say that similarity was again the principal yardstick for selection. Fedigan (1986: 45) excluded North American Indian societies 'because of their recency, geography, and specialization for a habitat unsuitable to agriculture,' i.e. because of the amount of dissimilarity, regardless of it was relevant or not. McGrew (1992: 123) believed that 'the best comparison would be of the closest living relations in the closest approximation to the environment of hominization'. Ideally, such an analogue would satisfy six criteria: 'sympatry, pristinity, simultaneity, methodological identity, longevity and comprehensiveness.' The idea that a certain distance between source and target analogues might be productive was never considered. On the contrary, one had to find 'the closest living approximations' (McGrew 1992: 121).

The discrepancy with contemporary ethnoarchaeology could not have been more manifest. Whereas authors like Yellen, Binford, Gould, Schiffer and Whitelaw sought to go beyond the straightforward transfer of properties from a single ethnographic source in order to study dynamic processes in the present, several primatologists were convinced that finding the bottomline of humanity was the most appropriate way of using contemporary ethnographic analogues. It is important to stress that those primatologists who dealt with ethnographic sources were not marginalized mavericks, but key figures within their discipline.

Fedigan's essay was solicited by the board of the prestigious *Annual Review of Anthropology*, after her *Primate Paradigms* (1982) had achieved textbook status in much Anglo-American primatology. McGrew's *Chimpanzee Material Culture* was reprinted twice and received the 1996 W.W. Howells Book Prize of the American Anthropological Association; it certainly made McGrew the world authority on chimpanzee tool use for many years to come. Hewes was an *éminence grise* in discussions on the origin of language, especially renowned for his theory on the gestural origin of language.

The most elaborate use of an ethnographic model in a primatological context occurred with R.B. Lee's study of the !Kung. Lee, though trained as an ethnographer, was professionally and institutionally related with the human origins programme and primatological work of Washburn and DeVore. As an undergraduate from Toronto who had majored in anthropology and philosophy, he became interested in human evolution and primate behaviour. He recalls:

A paper ("Primate Behavior and the Origin of Incest") I gave at the 1960 meeting of the American Anthropological Association led to a meeting with Sherwood Washburn and Irven DeVore and an invitation to study primate behavior with them at the University of California, Berkeley. Soon after settling in Berkeley, I realized that my long-term research interest lay in studying people, not primates. My hunch was that research on contemporary hunter-gatherer groups—subject to critical safeguards—could provide a basis for models of the evolution of human behavior. (Lee 1979: 9)

As a consequence, his subsequent research on the !Kung took place within a disciplinary and institutional context of observing primate behaviour. Washburn and DeVore were his teachers at Berkeley, and DeVore became the supervisor who repeatedly visited him during fieldwork in the Kalahari. With DeVore studying the baboons and Lee the Bushmen, the general categories 'monkeys' and 'man' which headed Washburn and Avis' comparative table in 1958 were substantiated in less than ten years by the fieldwork of two of Washburn doctoral students. And it were those two students who, between fieldwork sessions, would organize one of the most influential meetings in the history of twentieth-century human origins research: the 1966 Man the Hunter symposium.

Despite Lee's embeddedness in primatology, he also diverged from the field's climate of opinion with regard to the use of ethnographic analogy. Unlike Fedigan, McGrew, and Hewes, he refrained from looking for the bottomline of human forager adaptations. In fact, his choice for the !Kung was originally given in by J. Desmond Clark, the British archaeologist who had worked for nearly twenty years at the Rhodes-Livingstone Museum in Northern Rhodesia (Zambia) and who had joined the staff at Berkeley in 1961: 'Clark was instrumental in focusing our attention on Southern Africa as a research locale and in particular on the !Kung San, a hunting and gathering people in the northern Kalahari Desert' (Lee 1979: 9; cf. Clark 1986; 1994 and Cooke, Harris and Harris 1987). Practical considerations such as the relative ease to access a part of the British empire (Botswana was still the Bechuanaland Protectorate in 1963 when Lee began), the mediation of a Cambridge alumnus like Desmond Clark and the colonial network of English-

speaking administrators and scholars had a larger impact on the decision to study the !Kung than any expectations of finding the least complex society of hunter-gatherers. Interestingly, these were the very same reasons which had allowed Washburn to observe baboons in East-Africa, and just as the easily observable baboons eventually turned into generalized representatives of the earliest hominid past and the primates *par excellence*, so the !Kung too became an icon, even well beyond the strictly academic circles, of the foraging mode of production in the present as well as in the past. They became the hunter-gatherers *par excellence*.

Though Lee was explicitly ‘looking at a contemporary hunting and gathering society from an evolutionary perspective’ (1979: xvii), throughout his study he sought to avoid ‘the implicit racism and biological reductionism of earlier anthropological work’ (xvii)—a reluctance close to the archaeological position on that matter. An anthropologist by training, he was much better aware of the dangers of social evolutionism than his fellow primatologists: ‘Many nineteenth-century writers had treated contemporary “savages” as “living fossils” or “missing links,” an approach that had become thoroughly discredited’ (xvii). He favoured an approach which allowed to study a great deal of social variables ‘without doing violence to the absolutely crucial recognition of the uniqueness of human culture’ (xvii). Lee insisted on several occasions on the fact that ‘the hunters are not living fossils: they are humans like ourselves with a history as long as the history of any other human group’ (1), indeed, ‘they were not missing links; they were as human as we.’ (432).

Yet how could the emphasis on their particularity and humanity be reconciled with an evolutionary ambition of drawing implications about the earliest human foragers? First, Lee believed, contemporary hunter-gatherers, no matter where they lived, all shared a core of features, ‘and this core of features represents the basic human adaptation stripped of the accretions and complications brought about by agriculture, urbanization, advanced technology, and national and class conflict’ (1979: 2). Underneath the assumedly thin layer of recent impacts and contacts with the modern world it was still possible, according to him, to find the essence of the hunting-gathering way of life, no matter where on earth. Because the core idea of the Man the Hunter conference was still out of the question, hunter-gatherers, even those from the modern world, formed a unitary and universal category that echoed humanity’s earliest adaptation. Inuit, Australian aborigines and Kalahari San were ‘among the few remaining representatives of a way of life that was, until 10,000 years ago, a human universal’ (1). Secondly, Lee urged that analogies had to be more than simple projections:

We will make no progress in this area by simply applying specific ethnographic data to archaeological sites. [...] Though specific ethnographic data may be of little use in reconstruction, if we can discern the principles underlying foraging behavior in all its variability, we can apply these principles to more dynamic models of foraging societies past and present. (Lee 1979: 433-4; original italics)

In order to find and apply these underlying principles, Lee drew upon two theoretical stances: uniformitarianism and Marxism. The uniformitarian approach provided ‘parameters within which key variables fluctuate, and the knowledge of their articulations allows us to test models of group structure and behavior of prehistoric foragers against the behavior of contemporary foragers’ (436). This came down to understanding causation within the present source that could be extrapolated to the past. It was therefore most appropriate for more mechanical issues such as caloric requirements, reproductive rates and components of ecological systems. For cultural practices beyond ecological behaviour, Marxism provided a better framework: indeed, since the ideological superstructure was determined by the mode of production, a thorough knowledge of the infrastructure allowed to make predictions on a society’s ‘higher’ realms.³ The cultural practice of sharing, for instance, might not be visible in the archaeological record, but by being strongly related to the foraging mode of production, it made sense to postulate it for past hunter-gatherers as well. The same held true for political egalitarianism, spatial mobility, and a particular sexual division of labour: archaeologically invisible, but structurally related to the hunting-gathering mode of existence. There was a causal link between ‘the foraging mode of production and its associated superstructure’ (434).

After having decried ‘the lack of a clarity about the methodology of ethnographic inference’ (433), Lee presented an argument that was more subtle than anything presented in primatological circles. In fact, his refusal to draw formal analogies and his insistence to understand underlying principles came close to what ethnoarchaeologists were working on. However, at the end of the day, his suggestions remained too vague, and perhaps too theoretical, to make any serious impact. It was all well and good to invoke conceptual systems like uniformitarianism and Marxism, in the absence of any worked-out examples or precise guidelines, the prospect of drawing analogies about prehistory remained fairly idle. Whereas ethnoarchaeologists developed a middle-range theory, Lee’s work remained strictly confined to general theory. The final chapter of his impressive ethnography *The !Kung San* (1979) did not meet the promise to shed light upon human evolution, but simply drew together a number of generalizations about !Kung society and sometimes even indicated them as universal forager truths. Whereas Lee consciously avoided facile projections, his universal and reified category of hunter-gatherers as well as his involuntary portrayal of the !Kung as its prototypical exemplar gave rise to a powerful image of human ancestry. This is perhaps the tragedy of !Kung ethnography: originally started as a way to avoid projective reasoning, it eventually turned into the most essentialist definition of the hunting-gathering way of life. Just like the Tasmanians, the Mrabri or the Tasaday, the !Kung came to be considered as one of those human groups ‘at the absolute minimum of complexity’, and in this even the most popular one. Lee had not only been institutionally embedded in primatology; his research subjects, the

³ Lee’s view of Marxism is in fact a form of infrastructural determinism and comes close to Harris’ so-called ‘vulgar materialism’.

!Kung, had also undergone a ‘primateological’ treatment of becoming a best possible model.

Divergent debates

Why is it that two contemporary disciplines with related research themes developed in such opposite directions? Why did archaeology and primatology diverge so vastly from each other, especially from the mid-1970s on? How is it possible that both fields invoked such different applications of analogy to deal with what was, after all, a similar question? It might have been easy enough to explain diachronic discontinuity within archaeology as a result of an ongoing dialogue with the disciplinary past and theoretical re-orientation; it is much more difficult to account for synchronic discontinuity between archaeology and primatology in such terms.

In the first decades of postwar origins research, however, there had been considerable contact and exchange of ideas between people studying primates and those studying the archaeological record. The first modern primate field studies emerged as a reply to an archaeological problem. Washburn started observing baboons right after the Pan-African Congress in 1955 in order to verify Dart’s claims for an osteodontokeratic culture. Goodall had been sent into the field on Leakey’s instigation to find out about early hominids by looking at chimpanzees. Initially, primatology had played an ancillary role to archaeology and practitioners of both fields were in regular contact with each other at occasions like the Pan-African Congresses and the Man the Hunter conference. By the late 1960s, however, primatology emancipated into a field of its own. There was an enormous expansion of fieldwork on monkeys and apes, because less than a handful of species had thus far been studied in depth. Apart from a young discipline’s necessity to document in a nearly ethnographical fashion the diversity of primate behaviour, the 1970s were characterized by the growing awareness that the very research object of this nascent field was rapidly vanishing. Here was a young and promising field, but its formal object was already close to extinction! For many primatologists, therefore, there was not a moment to lose; fieldwork became more important than keeping up with scholarly debates, especially in other disciplines. Kortlandt (1986: 127) sarcastically noted that ‘at present, too many field workers still spend far too much time in the field and far too little in the library.’

Yet the history of primatology is also one of growing involvement with primates. Once distant automatons that were identified by numbers, monkeys and apes increasingly received personal names, affection and respect from their observers. The objects became subjects and the neutral ‘it’ was replaced by ‘he’ or ‘she’ in accounts of their behaviour. It is remarkable how the notion of friendship was a regular trope in titles of popular accounts: *My Friend Toto: The Adventures of a Chimpanzee* (Kearton 1925); *My Monkey Friends* (Russell 1938); *My Friends the Baboons* (Marais 1939); *My Friends the Apes* (Benchley 1944); *My Friend the Chimpanzee* (Oberjohann 1957); *My Friends the Wild Chimpanzees* (Goodall 1967) and ‘Making friends with mountain gorillas’ (Fossey 1970). Despite the formulaic repetition and the rhetoric of privileged access it implied, the appeal to amicability certainly marked a difference with Du Chaillu’s *chasse*

au gorille, the cold eye of behaviourism, and even DeVore's socioecology. As the century moved on, nonhuman primates were no longer simply observed but also bestowed with affection, first as pets, later as wild species. The awareness that many primate species were severely endangered started to dawn from the mid-century onwards. This dim reality did not only dispossess primatology of its object of research and its *raison d'être*, but also bereft practitioners from their objects of affection. Fieldworkers turned more often into lobbyists than logicians. All three 'Leakey girls' launched political campaigns and conservation acts against habitat destruction (Galdikas in Kalimantan), poaching (Fossey in Rwanda) and hunting (Goodall in Tanzania). Next to these 'primates', several primatologists took on an active role in conservation, to the extent that the issue of conservation has now become heavily institutionalized into the discipline.⁴ Defending the lives and the rights of the nonhuman primates was probably more pressing, both in terms of time and emotions, than delving into technical debates, especially on such finicky themes like the proper use of analogy. What's more, claiming that the species you studied was highly relevant to an inquiry into human origins was perhaps the best way to secure some protective measures for it. A similar political commitment prevented the advocates of the chimp and bonobo model to address the logic of analogy. It would lead too far to detail the metaphorical linkage between the women's right movement and primate conservation efforts (Haraway 1989), but both consisted of a defence of the suppressed against relentless exploitation by androcentric or industrial powers. The chimp and bonobo advocates were no fieldworkers, but the reports about behaviour in the wild responded so adequately to their aspirations that they had little attention to the methodological problem of analogy and the way it was discussed in the field of processual ethnoarchaeology—a field, moreover, that was heavily male-dominated and where the boasting polemics were far removed from the *herrschaftsfreie Dialog* required by feminists. The result was that most, if not all primate models were formulated with little respect for what was going on in a related field like archaeology, both in terms of its data and its methods.

Archaeologists were also responsible for the discrepancy with primate studies. At the same time primatologists turned to the woods to study behavioural patterns of animals at the verge of extinction, the former went to the arctic and arid regions to observe contemporary hunter-gatherers. One of the great incentives of ethnoarchaeology, apart from its theoretical promises, had also been the urgency to document a foraging lifestyle that was rapidly disappearing through modern impacts. Binford and Yellen, for instance, astutely realized that they were studying cultural systems that had been in existence for centuries and would soon belong to the past, as they were already undergoing severe modification. Ethnoarchaeologists, too, had a narrowly circumscribed theoretical and empirical focus which made them rather insensitive to work undertaken elsewhere. While much interesting fieldwork in primatology was already being done, Yellen (1977: 3) still believed

⁴ All major primatological conferences schedule sessions on conservation, most publications sketch the endangered state of the species studied, and the *International Journal of Primatology* has even a specific logo (an aye-aye holding a 'vivamus' sign) to accompany articles which describe an endangered primate species.

that models from a related field like animal ethology were ‘theoretically applicable but practically difficult to apply’.

On top of that, the speculative tenor of debate in primate-based origin studies must have met with resistance and disapproval in ethnoarchaeological circles were the exigencies of factual demonstration became increasingly stringent. The sweeping narratives, the grand evolutionary scenarios, and the often gratuitous just-so stories, all based on single-species models and prime-mover explanations, must have raised more than one ethnoarchaeologist’s eyebrows. Some primatologists have argued that the social sciences, including anthropology and archaeology, have been reluctant to draw upon primate studies because of ‘a general disposition to discount explanations and accounts of human behavior that smack of what social scientists call “biology”’ (Loy and Peters 1991: 11). Yet what hunter-gatherer ethnoarchaeologists wanted was not a *less* mechanist theory, but one that was even *more*. Primate models were not too reductionist, but too impressionistic. Like proponents of a chimp model, they rejected the master narrative of Man the Hunter, but that rejection was based on taphonomy, not on feminism.

Consequently, the interest for primate behaviour was next to minimal among most ethnoarchaeologists. Their publications were extremely poor in references to primate studies. If they turned to ethology, it happened mostly in the context of formation studies. Binford studied the effects of bone gnawing by dogs, Brain observed how goats, porcupines and leopards affected bone assemblages. They worked on such minute patterns like the impact of canine pressure on the epiphysis of a caribou’s femur. The archaeological interest in animal behaviour occurred first of all in a research context of how organisms altered the fossil record post-depositionally, less in how they could be modelled as depositional agents.

Looking at primate modelling and ethnoarchaeology in the late 1970s and early 80s—the decades in which both reached their classical stage—we observe two introverted debates regarding the use of contemporary sources to understand the past. Whereas the one relied on the number of similarities to establish holistic projections, the other relied on the quality of understood causes to draw piecemeal transfers. The one valued analogues in terms of proximity, the other in terms of relevance. Whereas one was documenting amazing patterns of behaviour among often endangered species, the other was describing modern forager systems and devised a framework for using such contemporary evidence. In terms of reconstructing early hominid sociality, it seemed as if one group of scholars had relevant data and another group relevant methods. Both were separated by a disciplinary gap. Mutual lack of understanding and the absence of interdisciplinary communication thus impeded the potentially fruitful exchange of research results. The awareness that this was rather regrettable started to dawn in the late 1980s and it was again the Kinzey volume which paved the way: Potts criticized the primate modellers’ lack of attention for archaeological and geological evidence, Tooby and DeVore called for conceptual models and Strum and Mitchell’s insisted on studying processes rather than superficial similarities. This must have sounded familiar to ethnoarchaeologists brought up with the analogy discussion in their field. Here was at least an occasion of possible convergence. ‘Genuine

interdisciplinary collaboration,' McGrew observed some years later, 'between primatology, especially of the African apes, and palaeoanthropology, especially in archaeology and ecology, is long overdue' (in Sept 1992: 197). Although this necessity was worded by a primatologist, in general archaeologists have been more prepared to bridge the disciplinary hiatus. In the decade subsequent to Kinzey volume, a younger generation of primatologists stuck tenaciously to the value of referential chimp models, but in archaeology a new interest for primate behaviour emerged: Sept (1992; 1998) and Joulian (1994; 1996) went to the field to do ethoarchaeology, whereas Palaeolithic archaeologists like Mithen (1994) and Steele and Shennan (1996) have started to use the available primatological literature, even for more recent periods like the European Palaeolithic.

Primate modelling and the comparative method

Lubbock's famous statement (1865: 445-6; cf. *supra* p. 67-8) on the quest for the lowest savage can be easily modified to describe the primatological debate on hunter-gatherers:

Primate modellers have varied a good deal in opinion as to the society of hunter-gatherers which is entitled to the unenviable reputation of laying at the absolute minimum of complexity. Hewes was decidedly in favor, if I may so say, of the Mrabri and the Tasaday; Fedigan maintained that the Bushmen are the least complex; while McGrew voted for the Australians and Tasmanians.

It suffices to change 'travellers and naturalists' into 'primate modellers'; 'the race of savages' into 'the society of hunter-gatherers'; 'the lowest in scale of civilisation' into 'the absolute minimum of complexity'; the names of Victorian naturalists into the names of contemporary primatologists; and the Fuegian into the Mrabri and the Tasaday. What we get from this stylistic surgery is a fair portrayal of the debate in primatology on the use of ethnographic sources. Terms and names may have changed, but concepts have not. It takes therefore little imagination to see how related the primate debate is to the nineteenth-century comparative method, especially when they both talk about the same societies: the Bushmen and the Tasmanians are now as highly valued as sources for modelling as they were a century ago. The question becomes more challenging when we talk about entirely different sources like primates.

Proximity, privilege, projection and paradoxes

Comparing the logical analyses of the arguments put forward by sociocultural evolutionists and primatologists, a number of parallelisms cannot go unnoticed, especially between the most classical examples like Morgan's, Sollas' and Tylor's later reasonings, on the one hand, and the 'mature' baboon, chimp, bonobo, and neo-referential models, on the other. It comes as no surprise that *similarity* was highly valued by both as the most important criterion for assessing the validity of a contemporary source. Tasmanians and chimpanzees were both defended on the basis of the numerous resemblances they manifested with the observed and

assumed properties of the target analogue. Bushmen and baboons were invoked as they seemed to faithfully represent in the present what had been lost in the past. Against McLennan's observation of 'such similarity, so many correspondences, so much sameness' among savages (1865: 3) stood Tanner's insistence that chimps and humans were 'extremely similar and evidently closely related' (1987: 7). The impression of immediate likenesses turned the amount of resemblance into the decisive and even only criterion for analogical reasoning, at the expense of discussing the underlying *relevance* (which was considered redundant in the light of such manifest similarity) and of avoiding the aspect of *dissimilarity* (which was experienced as automatically invalidating the argument).

In this context it is useful to recall, again, the experiment cognitive psychologists devised to investigate the process of analogical reasoning, the experiment in which students were asked for advice about American military invention in a fictitious situation which, though identical, in one case superficially resembled World War II and in another Vietnam. Since respondents to the first case favoured intervention but the ones to the second advised against, the researchers concluded that surface similarity played a crucial role at the level of source selection. There is of course a difference between short-term individual *bricolage* and long-term collective reasoning. But we have seen how the classical episode of primate modelling was heavily concerned with selecting the right source, in particular regarding the chimpanzee and bonobo model and classical Victorian evolutionism had a great interest in finding an adequate representative for evolutionary stages, in particular for the lowest level of savagery. This obsession entailed a fascination with immediate similarity, regardless of its weighed relevance.

A logical consequence of this centrality of similarity was that the best source had to be the one closest to the target. If the amount of similarity is what matters, it follows that certain sources from the present world are closer. Proximity to the target determined the quality of the source. The *number* and *variety* of source contexts became totally inferior to the criterion of similarity in such reasoning from proximity. Hence the acrimonious debates in postwar primatology to find the best model; hence also the efforts in sociocultural evolutionism to find the lowest savage. What we see in these disputes is a strong disregard for using more than one source context at the time. Rather than studying the target under consideration (early hominid sociality, origins of humanity) from a multitude of angles and imported analogues, both primatologists and sociocultural evolutionists preferred to privilege one manifest source analogue above the others. Contemporary savages and nonhuman primates were seen as 'windows to the past', but only one window was opened at the time. The perspective of a more variegated, but necessarily more complicated view achieved by opening several shutters at once was only rarely explored.

Instead, this closest source served as the basis for wholesale projections from the present onto the past. Entire stages of prehistory were explained not by arguments derived from the privileged source, but by descriptions of it. The near-to-equal source thus replaced the obscure target; visualization being more important than argumentation. Sollas' lengthy exposés on the rituals of Australian aborigines were echoed less than

a century later by Tanner's extensive evocations of chimpanzee life: in the one case it served the shed light on Neanderthals, in the other on the last common ancestor of humans and chimpanzees, but in both cases the argument consisted of downright projections. It was to no avail that cognitive psychologists argued that 'analogies should enhance thinking, not substitute for it' (Holyoak and Thagard 1995: 133). As a consequence, the *weight* of the inferences generally outbalanced the weight of the premises, because the trust in similarity led to overbearing reconstructions that went beyond the available evidence. The 99 % of genetic overlap between the genus *Pan* and the genus *Homo* had given rise to such vast projections that both chimp species could replace the actual common ancestor. Let it be an impressive percentage of genetic resemblance, it is even more impressive to see the amount of dissimilarity in anatomy, behaviour, and cognition which seems to spring from the remaining percent. There is much left in the final percent, or else there is much that is non-genetic. Let us not forget that 74 % of the known human genome can be found with *Caenorhabditis elegans*, a rudimentary worm who was the first of all zoological species to have its genome integrally mapped!⁵ In the absence of considering the meaning or the relevance of this genetic closeness, one shouldn't be misled by mere numerical rhetoric. A similar criticism could also be levelled against Tylor's equation of Tasmanians with Neanderthals. One antipodal side-scraper and a couple of thick eyebrow ridges were apparently sufficient to draw far-reaching inferences on the Mousterian. Of course, technological ingenuity (or the lack thereof) and anatomical peculiarities of assumed primitiveness were considered by the evolutionists as crucial cultural markers, but it was still quite a daunting step to infer equivalence or even identity from such arbitrary markers. Blinded by the amount of immediate resemblance, scholars have thus often attributed far too much weight to the predicted similarity in comparison to the observed similarity.

Two interesting paradoxes result from this reasoning from proximity. First, whereas one might be able to indicate the closest source among the available alternatives, one has still no idea of the remaining distance to the actual target itself, especially when that target is poorly known (this is the whole point one wants to resolve!). Bonobos are almost certainly closer to the last hominid-pongid common ancestor than baboons are, but there is no way of telling of *how* close they are. This is the irony of such single-species or single-savages reasoning: it tries to increase the aspects of similarity with the target, but for many aspects the target's features are simply unknown (because they are not in the archaeological record). A first paradox can thus be phrased: reasoning from proximity comes down to finding similarities when such similarities cannot even be established because simple comparisons cannot be made. In order to make such comparisons, one needs to know more about the target, so a present source might be invoked, but that requires the assessment of similarity and how are we to measure similarity in this case? By invoking a new source? Etcetera, etcetera. In the absence of considering

5 *C. elegans* was mapped by the end of 1998; the human genome should be mapped by 2003. The mapping of *Drosophila melanogaster*, the fruit fly so intensively studied by geneticists for almost a century, was completed by March 2000. In this case, too, daunting amounts of similarity in terms of percentage with humans were noted (which was said to be around 60 percent).

relevance, we turn into a *regressum ad infinitum*. This, therefore, is the *paradox of infinity*.

One way out of this paradox comes down to showing that the present source is indeed quite close to the prehistoric target because it has not changed very much since that time. This was what the entire debate between the evolutionists and Argyll centred upon: against the Duke's claim that modern primitives had come to their present state because of unfortunate histories of degradation and rejection, the evolutionists maintained that savages had always been like this, that they were people without history, and that the factor of time was of no importance in describing their natural and universal state. It is no coincidence that those authors who granted attention to the particular histories of the savage tribes (first Argyll, later Boas) were also the ones who refrained from using the comparative method and even fiercely criticized it. It is no coincidence either that the young Tylor of the *Researches* still stressed the overall importance of history and was careful with cross-cultural comparison; only later, in the *Origins of Culture*, as a full-blown evolutionist did he disregard such thing like historical exactness or geographic precision. An awareness of history could not be reconciled with the need for a projectable source. This was also true in the case of primatology. Obviously, there was the long-standing thought that only humans had history and animals none. Whereas humanity was perceived as dynamic, animals were regarded as static, out of time, immutable, or mutable only at very slow rates and beyond their proper will. Washburn could organize his baboon evidence as the primate basis from which early hominids had departed since primates 'have changed far less than has man' (Washburn and Avis 1958: 422). That contemporary baboons might be very different from Plio-Pleistocene ones, let alone from very early hominids, did not matter to him. Similarly, Tanner took chimpanzees as her starting point, literally calling them 'conservative' (1981: 65). The 'conservative' chimps thus stood on the same footing as the 'arrested development' of the Tasmanians: they continued into the present a state of being from the past. Here, too, it is no coincidence that an author like De Waal who has stressed the importance of history to understand nonhuman primates obstinately refuses to advocate a referential model. History obstructs reasoning from proximity, it shows that there is at least one difference between source and target, i.e. time—and the effects this may cause. The denial of history was an ultimate, though by no means guaranteed, device to make the privileged source appear as effectively close to the target. It was invoked by both debates as an answer to the paradox of infinity.

A second, related logical problem comes down to the idea that one can never have enough resemblances. The amount of similarity can always be further augmented. The number of formal likenesses that is required to safely draw inferences can only be reached when similarity is absolute, yet this is the point where no further inferences can be drawn. This then is the second paradox of reasoning from proximity: similarity is never enough, and when it is enough, it becomes tautological. We can call this the *paradox of insufficiency*. Finding the perfect source analogue would mean finding a living australopithecine, a healthy last common ancestor, a brisk Neanderthal, or an energetic Tertiary man. Of course, primate

modellers and sociocultural evolutionists realized that such was not the case, but continued to believe that *ideally* the best model would still be an identical source. By thus acting *as if* the source equalled the target, they ran into the inevitable fallacy of the perfect analogy, i.e. the danger to reason from partial resemblance to total identity. This was obvious, as we have seen, with the chimpanzee and bonobo models. It also happened to Sollas, for instance, who had to argue that all his sources were historically related to his targets, only to be able to substitute the ones for the others. Tylor and Morgan, on the other hand, had to stress the psychic unity of mankind in order to be able to assemble widely dispersed sources as stand-ins for the stages of human evolution. Both debates fell into the fallacy of the perfect analogy to avoid the paradox of insufficiency.

Proximity led to privileging one source, privileging led to projective description, and this led to two paradoxes. The fact that proponents of the comparative method and of primate modelling reacted in similar ways to these paradoxes epitomizes the fundamental similarity between both debates.

Differences

Now we would be committing the same error as some of the evolutionists and primatologists have done if we only stressed similarities at the expense of differences in our comparison of the comparative method and primate modelling. Every comparison needs to assess the amount of discrepancy and to ascertain its meaning.

True, there are profound differences between both debates. First of all, there was much more variation within each. In primatology, the carnivore and the gelada model, as well as non-referential alternatives like phylogenetic comparison, behavioural ecology and ethnoarchaeology all escaped the reasoning from proximity. In sociocultural evolutionism, the early Lubbock differed strongly from the later classical comparative method. The above comparison has mainly focused on the most classical examples from both debates. But not without reason: these were not only the most outspoken cases, but were without exception the most popular and dominant ones. Lubbock's insistence on finding the common denominator between savage tribes met with no sympathy from his contemporaries at all. It was left in silence and was frequently undermined, even by Lubbock himself. Similarly, Jolly's gelada model or Sept's ethoarchaeology, interesting as they are, drew far less attention from the scientific community than the chimpanzee model. Against one author advocating an alternative way of reasoning from contemporary primates, there were at least ten defending a referential model based on one of the chimpanzee species. The section on geladas drew on three publications only, the one on chimp models dealt with at least a dozen of them (neo-referential models not even included). Even the alternatives brought up since the crisis of referential modelling are still outnumbered by the popularity of chimp models today. Despite all its good ideas, ethoarchaeology for instance is still only practised by a couple of devotees.

Interestingly, the ideas deviating from the general tendency in one debate could sometimes converge to an alternative brought up in another debate a century later. Lubbock's search for a common denominator was exactly echoed by the

Wrangham's call for phylogenetic comparison and the subsequent cladistics of behaviour: in both cases, looking for shared commonalities between several sources in the present was believed to be a more reliable procedure than picking out a single one for projection. Moreover, both worked from the premise that the source analogues were internally related, either by phylogeny or by psychic unity. Now it would lead too far to suggest that Lubbock was a forerunner of behavioural cladistics or that he was 'ahead of his time' or whatever other anachronism, but the fact that even exceptions to the dominant discourse structurally resemble each other affirms the profound likeness of those discourses.

The largest and most important difference between the comparative method and primate modelling concerns the concept of hierarchy. Whereas the nineteenth-century authors tried to correlate each developmental stage with contemporary ethnographic cases (regardless of whether these were tool types or entire tribes), thus arranging them into a hierarchy from simple to complex, primatologists never went so far. Primate modellers never claimed, for example, that the last common ancestor was a baboon, the earliest australopithecine a chimpanzee and *Homo habilis* a bonobo. Only Pfeiffer (1972) and Birdsell (1972) once suggested that our arboreal ancestors had first to be compared to chimps and later, when they had reached a terrestrial lifestyle, to baboons, but this was in the context of general textbooks and, compared to the elaborate sociocultural evolutionist schemes produced a century earlier, the notion was never seriously considered. In general evolutionists put contemporary sources on every rung of the ladder, primatologists only at the bottom rung. The difference is undeniably relevant as it relates to the respective research agendas: the Victorian scholars were above all concerned with the notion of *progress*, primatologists were more concerned with the notion of *origins*. The ones studied how human civilization had developed into the present, the others on how hominization had taken place in the past. Hierarchical classifications were an integral part of the socioevolutionist fabric, but had no role in the primatological debate. Primate modellers rarely worked on periods younger than the Plio-Pleistocene; once *Homo erectus* appeared on the scene, the use of primate models was withdrawn.

Similarity but no continuity

Far from being identical, the extent of dissimilarity between the two debates reminds us of the fact that primate modelling was not a twentieth-century replica of the Victorian comparative method. For all their similarities, there is very little reason to believe in a continuity of debate between nineteenth-century social science and postwar primatology, comparable to how early twentieth-century archaeology was articulated vis-à-vis the comparative method. Concepts or methods were not handed down from one century to the other, early primatologists did not seek explicit inspiration in the sociocultural evolutionism, there was no direct transfer of a discursive and conceptual apparatus between both debates. In fact, all historical evidence points to a strong caesura, both chronologically and disciplinarily, between the end of evolutionism in sociocultural anthropology in the 1910s and the emergence of the idea of a primate model in physical anthropology and

palaeoanthropology in the 1950s. Hooton, Washburn, and Leakey did not once refer to the comparative method when they stressed the opportunities of naturalistic observations on primates in the wild; instead they referred to what Yerkes' students had inaugurated in the Interbellum. In his earliest papers, Washburn cited the work of pre-war authorities like Yerkes, Zuckerman, Schultz, Hooton and Carpenter, never that of Lubbock, Tylor, Morgan or Sollas. Let us not forget that Washburn was a student of Hooton, and that he had participated in Schultz' and Carpenter's legendary Asiatic Primate Expedition of 1937. Hooton and Schultz had been influential in his anatomical work, Carpenter in his behavioural studies. If a pedigree of intellectual influence can be traced it goes from DeVore to Washburn, from Washburn to Hooton and Carpenter, and from Carpenter to Yerkes. The first primate model was thus historically rooted in the pre-war traditions of primate anatomy, psychology and what Yerkes had baptized 'psychobiology', certainly not in Victorian sociocultural anthropology. The earliest primatologists picked up the thread which had been severed by the war, but they twisted it to resolve new questions: not the human psyche or embryo, but the human past was now to be elucidated by it. Indeed, reading through half a century of primatology, I only once came upon a sentence which explicitly referred to nineteenth-century evolutionism. When McGrew (1992: 196-7) described the wealth of accumulated data on chimpanzees, he wrote: 'Were A.H.L.F. Pitt-Rivers alive today he might note an uncanny replication of the cumulative ethnography of the nineteenth century.' Tellingly, this single reference to Victorian anthropology speaks only of a similarity in *data* accumulation, not in the formulation of methods, concepts, or theories. If primatologists discussed nineteenth-century scholarship, it was obviously Darwinism, not sociocultural evolutionism. Lubbock's description of primate tool use was lost into oblivion and remained unnoticed by Goodall; Huxley's summary on great ape nests was forgotten; and even Yerkes and Yerkes' quote from Jobson on the marching order of the baboons was unknown to DeVore. Goodall admitted: 'By the time systematic observations of tool-using came from Gombe those pioneering studies [by Yerkes and Kohler] had been largely forgotten' (1990: 15). Primatology was less concerned about already 'dead' historical evidence than about biological evidence that was rapidly dying out.

The resemblance between the comparative method and primate modelling should, therefore, be seen in terms of 'recurrent' rather than 'continuing' ideas.⁶ The mutual likenesses are very real but do not result from a direct, historical transmission of ideas. To use the terms sociocultural evolutionists would have preferred: the similarity resulted from 'independent invention' rather than 'historical connexion'. Primate modellers had re-invented a logic which incidentally had already been applied a century earlier. Or to deploy the vocabulary familiar to primate modellers: we had to do with 'analogy', resemblance caused by similar circumstances, rather than 'homology', resemblance caused by similar descent; with 'convergence' rather than 'divergence'.

6 The distinction between continuing and recurrent ideas was introduced by Maurice Mandelbaum as an emendation to Lovejoy's too monolithic concept of the unit-idea (see Wilson 1987: 198).

Yet what was the nature of this similarity? What was its meaning, its *relevance*, to use the term we have been using so frequently in this study? In both debates arguments were based on the amount of immediate resemblances, disregarded difference, relevance, and source variety, and resulted in disproportionate projections. But what did that imply? Are not most analogies based on simple similarities? Is the awareness of resemblance not the prime mover to unleash analogical thinking? In sum, is the *logical* similarity observed between both debates also a *structural* one? This question goes straight to the finale of this study.

Conclusion

Ethnographic and primatological analogies have played a decisive role in gaining an understanding of prehistory during the last two centuries. These external sources have often been beneficial to shed light on a range of themes that are badly documented in the archaeological and fossil record. At the same time, analogical reasoning has repeatedly tainted and sometimes even distorted an interpretation of the past, especially when too much confidence was given to particular source analogues. The proper use of analogy, therefore, consists of a critical appreciation of its strengths and weaknesses by finding a balance between undue confidence and overdone scepticism.

The preceding chapters have charted a history of analogical reasoning as used by students of prehistory since the early nineteenth century. This was done by focusing on three large debates in Anglo-Saxon science—the comparative method, ethnoarchaeology and primate modelling. All major texts from the history of these debates have been subjected to a logical analysis by weighing their often implicit arguments against a sixfold criterion of analogical strength. This has allowed to document and to compare a variety of arguments used, as well as to delineate a number of larger tendencies (figure 22). Firstly, ethnoarchaeological analogies, though often called neo-evolutionist, are in logical terms totally unconnected to the parallels of Victorian sociocultural evolutionism. Since this distinction was explicitly sought by twentieth-century archaeologists in response to the comparative method, we have to do with a relation of *diachronic discontinuity*. Secondly, the same ethnoarchaeological analogies also differed profoundly from the primate models which were developed at the same time. The absence of interdisciplinary exchanges between students of primate behaviour and hunter-gatherer ethnoarchaeologists have turned this relation into one of *synchronic discontinuity*. Thirdly, primate models as they developed after the Second World War logically resemble the ethnographic parallels of the nineteenth-century comparative method in a number of fundamental aspects. This relation is one of *diachronic continuity*.

Yet how should such diachronic continuity be understood? How is it possible that there is more affinity between two debates from separate disciplines in separate centuries than between debates that are either simultaneous or part of the same field? These questions bring to mind the discussion evoked in the introductory chapter between structuralist and historicist approaches to human origin studies. Structuralist historians of science, with their preference for a long-term perspective, would interpret the resemblances between primate modelling and the comparative method as sufficient reason for assuming continuity from the nineteenth to the twentieth century. According to them, the use of primate models in postwar human origins research would only be a slightly modified continuation of a Victorian and perhaps even older tradition of representing an imaginary other. Tanner and Zihlman would simply be walking in Morgan's and Sollas' footsteps, while Wrangham's phylogenetic comparison would be an echo of Lubbock's common denominator. More traditional historians of science, however, would point out that there is very little ground for such conclusion. After a study of the respective short-term contexts, they would rightly indicate the absence of clear-cut

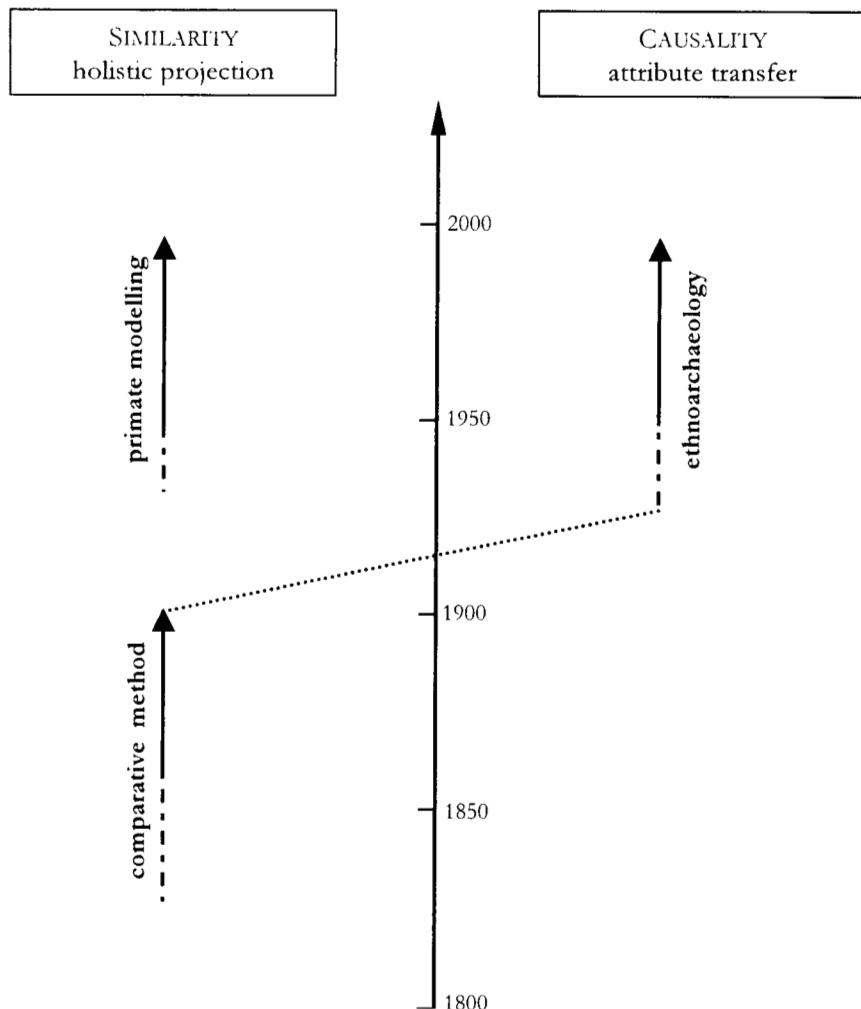


Figure 22. While ethnoarchaeology was an explicit move away from the similarity-based projective reasoning of Victorian social evolutionism, postwar primate modelling implicitly echoed the older comparative method. This resemblance was due to recurrent rather than ongoing ideas about the nature of creatures like primitives or primates.

transfers of methods and ideas via intellectual borrowing, personal tutorship, institutional affinity or any other mechanism. Indeed, all available evidence suggests that there was a break rather than a bridge between sociocultural evolutionism and the first primate model. Beyond the manifest reality of similarity, little seems to justify a belief in continuity.

What, then, is the importance of this similarity? Since it resulted from independent invention rather than historical connection something may be learned from the former's doctrine that 'like circumstances produce like results'.

The use of ethnographic parallels in the nineteenth century received an important impetus from the establishment of human antiquity (Chapter 2). Though Wilson and Nilsson had indicated its potentials before, it was only after 1860 that the comparative method developed into a systematic principle. Historians of Victorian science have often noted how the sudden expansion of humanity's existence on earth caused a 'revolution in ethnological time' (Trautmann 1992). Murray has rightly argued that the establishment of human antiquity saddled the sociocultural evolutionists with the need to find 'a human face for the Palaeolithic' (1992: 733). Stocking demonstrated how contemporary savagery was invoked to fill 'the void in cultural time' (1987: 178). The comparative method thus served to populate the long stretches of geological time with already familiar 'savages'. In fact, Grahame Clark (1953: 345) had already realized this when we wrote: 'The mid-Victorian anthropologists were confronted by an immense void, for many of them suddenly apprehended, and they merely did what any other scientists would have done under similar circumstances—they plugged the gap with hypotheses.' The elaboration of the comparative method was thus intimately related to the conceptual and temporal hiatus that had been blown by the picks at Brixham Cave.

Much the same holds true for the development of primate modelling. The introductory section of Chapter 4 has traced how the idea of a primate model emerged in the context of new fossil discoveries. The australopithecine hominids unearthed by Dart in South-Africa and the Leakeys in East-Africa were much older than anything hitherto found; and it was in response to this evidence that Washburn and Leakey stimulated DeVore and Goodall to study baboons and chimpanzees. When the first absolute datings (obtained through the potassium-argon method) proved these fossils to be extremely old, the need for a comprehensive understanding through careful use of external sources even increased.¹ This 'potassium-argon revolution' and the increasing number of early hominid fossils discovered during the 1960s were instrumental in sustaining an interest in primate behaviour. Without these finds, primate studies would not have developed the way they did. Paraphrasing Murray, one could say that palaeoanthropologists needed 'a primate face for the Plio-Pleistocene.' Whether the dig was in Devon, the Somme valley, Transvaal or Olduvai, visualizing the occupants of an enormously expanded human past was an important incentive for calling upon an external source.

Apart from a new definition of prehistoric time, these crucial chronological discoveries also implied a new definition of prehistoric humanity. The establishment of human antiquity implied that prehistoric man was not only very old, but also very primitive. Discovering a 'time indefinitely more remote' implied 'a condi-

1 The first such dating was published in 1961: it was the potassium-argon method applied to volcanic layer IB at the bottom of the Olduvai sequence where *Zinjanthropus* had been found and it gave an age of 1.75 million years. This caused quite a sensation as it was three times older than originally expected on geological grounds (Lewin 1997: 141). Parallel to the 'radiocarbon revolution' created by the application of ¹⁴C datings in the study of European prehistory (Renfrew 1973b), a 'potassium-argon revolution' thus took place in the study of the African Plio-Pleistocene. Radiometric dating fundamentally changed the perception of both recent and remote prehistory.

tion of man indefinitely lower' (McLennan 1869: 523). Most sociocultural evolutionists were not substantially influenced by Darwinism and the Neanderthal did not play a central role in their debates, but the idea that such primeval humans were closer to the animals was without question.² Similarly, the early hominid fossils discovered in Africa were recognized as much more ape-like. This was expected by evolutionary theory—it was not without reason that the word 'pithecus' appeared in their taxonomic name—but Dart's (1925) estimate of 460 cc for an adult australopithecine cranial capacity was quite below the 1000 cc attributed to *Pithecanthropus*, the oldest fossil until then, and much closer to a chimpanzee's 400 cc. Whether it was based on cultural level or cranial volume, the image of prehistoric humanity that emerged in both contexts was that of a hybrid between the human and the animal realm.

Faced with long stretches of time and with more animal-like ancestors, it was almost evident that scholars from both debates would turn to those sources in the present who were believed to occupy a liminal position between humanity and animality. Primitives and primates represented similar experiences of human-animal ambiguity the moment they were invoked as privileged external analogues. Victorian evolutionists, regardless of whether they endorsed Darwinism or not, commonly agreed that contemporary savages stood much closer to the animal kingdom than Europeans did. Lyell (1863: 96) could approvingly quote Humphry when saying that 'the inferior races of mankind exhibit proportions which are in many respects intermediate between the higher, or European, orders, and the monkeys'. Tylor (1871: I, 37) wrote: 'If the advance of culture be regarded as taking place along one general line, then existing savagery stands directly intermediate between animal and civilized life.' Pitt Rivers (1874: 8) believed that the psychological standard of primeval man could only have been 'at the level at which we find the highest of the lower animals that exist at the present time.' And Lubbock (1865: 446) went so far as to mention the opinion that 'monkeys are more human than Laplanders'.³ In physical, in cognitive and in cultural respect, the 'dark-skinned savage' was thought to approximate the nonhuman primates (Stocking 1968b; Gould 1981).

On the other hand, the postwar explosion of behavioural studies increasingly represented nonhuman primates as standing closer to humanity than other animal creatures. Baboons were said to be 'almost human' (Strum 1987), chimps were thought to be only 'in the shadow of man' (Goodall 1971), and bonobo cognition was considered 'at the brink of the human mind' (Savage-Rumbaugh and Lewin 1994). The repeated reference to humanity in titles of primatological monographs tends to obliterate that these are publications about animals. Of all the primates, great apes are the most outspoken boundary creatures between the human realm and the rest of the animal kingdom. Jane Goodall explicitly accorded such transitional status to the chimpanzee. 'Chimpanzees bridge the

2 Even the Neanderthal itself was from the 1860s to the 1890s still interpreted as an essentially human creature, albeit the very lowest form of it. Authors as diverse as Schaaffhausen, Huxley and de Quatrefages and Hamy (the first two were evolutionists, the latter two not) underlined that the Neanderthal was the oldest and lowest human race hitherto known (Van Reybrouck 1998a).

3 Lubbock made the most extreme statements in this respect. He also named the Hottentots 'the filthiest people in the world. We might even go farther, and say the filthiest animals' (1865: 338). The Fuegians were, in the words he quoted from Adolph Decker (1624), 'rather beasts than men' (1865: 432) and Bates was cited to stress that 'the monkeys lead in fact a life similar to that of the Paráruate Indians' (1865: 476). Secondary literature was thus invoked to stress the bestiality of contemporary savages.

gap between ‘us’ and ‘them’, she wrote (1990: 209): ‘Just as he is overshadowed by us, so the chimpanzee overshadows all other animals’ (Goodall 1971: 227). By being so close to humans, chimpanzees were believed to bridge the gap between animals and humans Goodall: ‘The chimpanzee is more like us than is any other living being’, not just in terms of anatomy and physiology, but especially in terms of ‘social behaviour, cognition, and emotionality’ (1990: 209). And McGrew (1992: 230) added that ‘much of what chimpanzees do is so close to human that the two are indistinguishable’. Haraway (1984: 77–8) has rightly argued that since ‘nonhuman primates are seen to exist at a crucial boundary between animal and human [...] they are privileged beings for understanding “nature” and “culture,” among the principal analytical categories Western people have used to theorize their histories and experience.’

Part-human, part-animal, the categorial ambiguity of primitives and primates did not only provoke complex cultural discourses of representation but provided a rich source for scientific speculation on the nature-culture transition. Victorian evolutionists came up with the most animal-like human; primate modellers looked for the most human-like animal. Even if there was no *historical continuity* between sociocultural evolutionism and primatology, there was at least a powerful *discursive continuity* with regard to the particular sources. Several of the research questions currently at stake in primatology (like tool use, language, and cognition) do not differ from favourite Victorian themes on the level of technological mastery, linguistic competence and mental capacities. The recent discussion about whether chimpanzees are by nature peaceful or aggressive sounds like an echo of the old debate on the noble or ignoble savage.⁴ The frequent tendency to evaluate primate behaviour from a human perspective has been called ‘speciesism’ (Noske 1989), as a modern version of nineteenth-century racism. And, finally, contemporary arguments for great ape rights have been likened to the Victorian case for the abolishment of slavery (Cavalieri and Singer 1993). Both in scientific and extra-scientific contexts, primitives and primates occupy related discursive positions.

Once these source analogues were selected, the notion of similarity became more important during subsequent histories of debate. Both the comparative method and primate modelling underwent a profound theoretical reorientation: degenerationism was to the one, what sociobiology was to the other. Both theories, though utterly different in their appreciation of analogy, led to a greater emphasis on the role of similarity. The degenerationist claim that modern savages did not parallel prehistoric people provoked an even greater insistence on their fundamental likenesses on the part of the evolutionists. Likewise, sociobiology’s rejection of the straightforward causality between environment and behaviour enhanced the necessity of documenting similarity, preferably in genetic terms: here, too, proximity became the basis for all further inferences. Marshalling all the available resemblances was thus the response to the new theoretical conditions imposed on

4 Goodall’s initial portrayal of a peaceful chimpanzee society was questioned with the observations of infanticide and structured aggression, both in the wild and in captivity (De Waal 1982). In recent years, the debate has been to the foreground of chimp studies (De Waal 1989; 1996; De Waal and Lanting 1997; Wrangham and Peterson 1997) De Waal’s (1996) book was subtitled ‘the origins of right and wrong in human and other animals’; Wrangham and Peterson’s (1997) book was subtitled ‘apes and the origins of human violence’. They read like a late-twentieth century version of the debate between Hobbes and Rousseau.

the use of external sources. As a result, primitives and primates increasingly began to be invoked for projective reasoning on account of their proximity.

By drawing upon entities from the nebulous but intriguing zone where the human-animal boundary was situated in their respective times, evolutionists and primatologists incorporated a set of analogues to deal with the ambivalent occupants of a recently blown temporal void. Subsequent polemical conditions forced them to stress immediate similarity between source and target and thus to privilege certain sources over others as proximate entities that could be projected. Without historical transmission, but through discursive convergence, the chimpanzee had become the Tasmanian of today.

A history of analogical reasoning in the nineteenth and twentieth-century prehistoric scholarship thus reveals a specific transition from primitives to primates. Of course, primates had been looked at before the 1950s and, of course, ‘primitives’ continued to be influential after that date (in ethnoarchaeology, in the work of Lee). But the practice of using these entities from the present world as contemporary ancestors that could be holistically projected into the past was largely restricted to the particular polemical contexts of the Victorian comparative method and postwar primate modelling. It was in these contexts too that similarity was required to become identity, that piecemeal transfers became wholesale projections, that visualization became more important than argumentation, and that the fallacies of tautology, circularity and self-fulfilling prophecy were frequently encountered.

The year 1955 stands out as the symbolic pivotal point for that transition. First of all because it was the year which saw the publication of Hooton’s defence of the remarkable article that coined the term primatology and stressed the superiority of primates over primitives for understanding human origins. It was also the year of the Third Pan-African Congress on Prehistory after which Washburn started to watch baboon behaviour, first on his own, later with DeVore. For Clark, who had organized the conference, it marked ‘a turning point’ in his career (Clark 1994: 11). His meeting with Washburn would lead to a move to Berkeley, and his eventual insistence that graduate student Lee should study the !Kung. In 1955 the foundations of a research programme on human origins were thus laid through a number of contingent factors that led to the influential research group in Berkeley.

But it was more than a matter of serendipity. The formulation of this research programme occurred against the background of a fundamental re-definition during the postwar years of what it meant to be human. The Universal Declaration of Human Rights, the UN charter and the Unesco statements on race had all appeared shortly before and implied a very different perspective on human variability than before. It promulgated the idea of ‘universal man’. Despite racial variation, humans were now said to possess an essential equality in terms of culture, cognition, and body. The term ‘race’ was increasingly avoided as a scientific concept in favour of terms like ‘population’ in biology and ‘culture’ in anthropology. Such a new perception of humanity affected the preference for primates as better source analogues than tribal societies. This is not a simple externalist explanation for the transition to primate models, because many anthropologists had been involved

with the writing of the Unesco declaration itself (science articulated as much politics as politics articulated science), and because the Unesco text did never dictate the use of primate models as such; this was an original solution that had emerged within the discipline itself. The point is *not* that early primatologists were simply influenced by the Unesco ideology but that their work partook in a wider intellectual movement of which the Unesco text formed another, conspicuous outcrop.

The year 1955 also witnessed the opening of Edward Steichen's monumental photograph exhibition *The Family of Man* in the New York Museum of Modern Art. It epitomized the changing contours of humanity's self-understanding. The exhibition consisted of a compilation of more than 500 pictures taken by a host of photographers in 70 different countries. The exhibition toured the world in the following years, stopping on its way in Brussels and Amsterdam and attracting more than nine million visitors. Mankind had become kinship; and the aspirations of this new cultural ethos was one of universal brotherhood. Ever since, as the Flemish poet Herman de Coninck described the effect of Steichen's exhibition, there was 'intimacy under the milky way'.

Epilogue

This work started with a century-old scientific institution, the Tervuren Museum in Belgium but ends with a brand-new one.

In the course of the last two years, I started to compile a small list of potential titles for my Ph.D. That the subtitle would be ‘a history of ethnographic and primate analogies in the study of prehistory’ was rapidly decided. The choice of the main title, however, took longer, being more of an aesthetic conundrum than anything else. I wanted a catchy phrase which would refer both to Victorian ethnographic parallels and to postwar primate models. I came up with melodious and—less melodious—suggestions like ‘Windows to the past’, ‘Ancient Analogies’ (or even ‘Ancestral Analogies’), ‘Living Fossils’ and the not very original one ‘Contemporary Ancestors’. For a while, I also thought about the euphonic ‘Living Links’, which had been the title of a PhD synopsis I wrote in November 1996.

Yet in April 1998, I read an essay on Frans de Waal in the Dutch weekly *Intermediair*. Apart from some self-glorifying optimism about the successes of Dutch science abroad, the article explained that De Waal had been appointed director to a new institute for studying great ape behaviour, genetics and human evolution in Atlanta. The institute had been baptized... *The Living Links*. ‘It is a play of words,’ the journalist quoted one of De Waal’s close collaborators, ‘whereas palaeontologists study missing links, we want to look at the behavior of our living relatives to learn more about our own evolution.’ It would have made a nice title, but it had already been used. Although it made my eventual choice of title easier, I regretted that one of my candidates had been involuntarily eliminated. Entitling my dissertation ‘Living Links’ could have given the impression that I was writing a historical legitimization of an institution which I had not even visited.

After a while, I started to think that the name of this institution was perhaps not accidental; that it precisely indicated what I was trying to say, namely that today nonhuman primates as ‘living links’ were the modern version of the nineteenth-century ‘contemporary ancestors’. Rather than rejecting one of my titles, the name of the centre in Atlanta confirmed the main hypothesis of this text. If the phrase could not appear on the first page of my dissertation, I thought, it should at least appear on the last.

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Curriculum Vitae

David Van Reybrouck (Bruges, 1971) was trained as an archaeologist at the universities of Leuven, Cambridge and Leiden. For more than twelve years, he was coeditor of Archaeological Dialogues. In 2011-12, he held the prestigious Cleveringa Chair at the University of Leiden.

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FROM PRIMITIVES TO PRIMATES

Where do our images of early hominids come from? In this fascinating in-depth study, David Van Reybrouck demonstrates how input from ethnography and primatology has deeply influenced our visions about the past from the 19th century to this day – often far beyond the available evidence. Victorian scholars were keen to look at contemporary Australian and Tasmanian aborigines to understand the enigmatic Neanderthal fossils. Likewise, today's primatologists debate to what extent bonobos, baboons or chimps may be regarded as stand-ins for early human ancestors. The belief that the contemporary world provides 'living links' still goes strong. Such primate models, Van Reybrouck argues, continue the highly problematic 'comparative method' of the Victorian times. He goes on to show how the field of ethnoarchaeology has succeeded in circumventing the major pitfalls of such analogical reasoning.

A truly interdisciplinary study, this work shows how scholars working in different fields can effectively improve their methods for interpreting the deep past by understanding the historical challenges of adjacent disciplines.

Overviewing two centuries of intellectual debate in fields as diverse as archaeology, ethnography and primatology, Van Reybrouck's book is one long plea for understanding the past on its own terms, rather than as facile projections from the present.

David Van Reybrouck (Bruges, 1971) was trained as an archaeologist at the universities of Leuven, Cambridge and Leiden. Before becoming a highly successful literary author (*The Plague, Mission, Congo...*), he worked as a historian of ideas. For more than twelve years, he was coeditor of *Archaeological Dialogues*. In 2011-12, he held the prestigious Cleveringa Chair at the University of Leiden.



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